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# ROYAL SOCIETY OF LONDON.

*From April* 12, 1888, *to June* 21, 1888.

VOL. XLIV.

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# PROCEEDINGS

OF

# THE ROYAL SOCIETY.

*April* 12, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

## THE BAKERIAN LECTURE.—"Suggestions on the Classification of the various Species of Heavenly Bodies." A Report to the Solar Physics Committee. Communicated at the request of the Committee. By J. NORMAN LOCKYER, F.R.S.

CONTENTS.

[Received March 21, 1888.]

## PART I.—PROBABLE ORIGIN OF SOME OF THE GROUPS.

### I. *Nebulæ.*

In a paper communicated to the Royal Society on November 15th, 1887, I showed that the nebulæ are composed of sparse meteorites, the collisions of which bring about a rise of temperature sufficient to render luminous one of their chief constituents—magnesium. This conclusion was arrived at from the facts that the chief nebula lines *are coincident in position* with the fluting and lines visible in the *bunsen burner when* magnesium is introduced, and that the fluting is

far brighter at that temperature than almost any other spectral line or fluting of any element whatever.

I suggested that the association or non-association of hydrogen lines with the lines due to the olivine constituents of the meteorites might be an indication of the greater or less sparseness of the swarm, the greatest sparseness being the condition defining fewest collisions, and therefore one least likely to show hydrogen. This suggestion was made partly because observations of comets and laboratory work have abundantly shown that great liability to collision in the one case, and increase of temperature in the other, are accompanied by the appearance of the carbon spectrum instead of the hydrogen spectrum.

The now demonstrated meteoric origin of these celestial bodies renders it needful to discuss the question in somewhat greater detail, with a view to classification; and to do this thoroughly it is requisite that we should study the rich store of facts which chiefly Sir William Herschel's labours have placed before us regarding the various forms of nebulæ, in order to ascertain what light, if any, the new view throws on their development.

To do this the treatment must be vastly different from that—the only one we can pursue—utilised in the case of the stars, the images of all, or nearly all, of which appear to us as points of light more or less minute; while, in the case of the nebulæ, forms of the most definite and, in many cases, of the most fantastic kind, have been long recognised as among their chief characteristics.

It will at once be evident that since the luminosity of the meteorites depends upon collisions, the light from them, and from the glow of the gases produced from them, can only come from those parts of a meteor-swarm in which collisions are going on. Visibility is not the only criterion of the existence of matter in space; dark bodies may exist in all parts of space, but visibility in any part of the heavens means, not only matter, but collisions, or the radiation of a mass of vapour produced at some time or other by collisions. The appearances which these bodies present to us may bear little relation to their actual form, but may represent merely surfaces, or loci of disturbance.

It seemed proper, then, that I should seek to determine whether the view I have put forward explains the phenomena as satisfactorily as they have been explained by old ones, and, whether, indeed, it can go further and make some points clear which before were dark.

To do this it is not necessary in the present paper to dwell at any great length either on those appearances which were termed *nebulosities* by Sir William Herschel or on irregular nebulæ generally; but it must be remarked that the very great extension of the former which there is little reason to doubt will be vastly increased by

increase of optical power and improvement in observing conditions and stations—may be held to strengthen the view that space is really a meteoritic *plenum*, while the forms indicate motions and crossings and interpenetrations of streams or sheets, the brighter portions being due to a greater number of collisions per unit volume.

From this point of view it is also possible that many stars, instead of being true condensed swarms due to the nebulous development to which we have referred, are simply appearances produced by the intersection of streams of meteorites. They are, then, referable to an intensification of the conditions which gave rise to the brighter appearances recorded by Herschel here and there in his diffused nebulosities. The nebulous appendages sometimes seen in connexion with stars strengthen this view.

When we come to the more regular forms we find that they may be generalised into three groups, according as the formative action seems working towards a centre; round a centre in a plane or nearly so; or in one direction only. As a result we have globular, spheroidal, and cometic nebulæ. I propose to deal with each in turn.

*Globular Nebulæ.*

The remarkable appearance presented by the so-called planetary nebulæ requires that I should refer to them in some detail. Sir William Herschel does not describe them at any great length, but in his paper on "Nebulous Stars" he alludes to the planetary nebulosity which in many cases is accompanied by a star in the centre, and finally comes to the conclusion that "the nebulosity about the star is not of a starry nature" ('Phil. Trans.,' vol. 81, 1791, p. 73.)

Sir John Herschel, in his valuable memoir published in 'Phil. Trans.,' 1833, describes them as "hollow shells" (p. 500). It was so difficult to explain anything like their appearance by ordinary ideas of stellar condensation that Arago, as quoted by Nichol ('Architecture of the Heavens,' p. 86), abandoning altogether the idea that they represented clusters of stars or partook in any wise of a stellar constitution, imagined them as hollow spherical envelopes, in substance cloudy and opaque, or rather semi-transparent; a brilliant body invisible in the centre illuminating this spherical film, so that it was made visible by virtue of light coming through it and scattered by reflection from its atoms or molecules.

Lord Rosse ('Phil. Trans.,' vol. 140, 1850, p. 507) records that nearly all the planetary nebulæ which he had observed up to that time had been found to be perforated. In only one case was a perforation not detected, but in this ansæ were observed, introducing into the subject for the first time the idea of nebulous bodies resembling to a certain extent the planet Saturn. But Lord Rosse, although he thus disposed of the idea of Arago, still considered that the annular

ɔulæ were really hollow shells, the perforation indicating an appa-tly transparent centre.

Huggins and Miller subsequently suggested that the phenomena resented by the planetary nebulæ might be explained without erence to the supposition of a shell (or a flat disk) if we consider m to be masses of glowing gas, the whole mass of the gas being andescent, so that only a luminous surface would be visible ('Phil. ans,' vol. 154, 1864, p. 442).

It will be seen that all these hypotheses are mutually destructive; t it is right that I should state, in referring to the last one, that the nonstration that these bodies are not masses of glowing gas merely been rendered possible by observations of spectra which were not ilable to Dr. Huggins when his important discovery of the bright-e spectrum of nebulæ was given to the world.

It remains, then, to see whether the meteoritic hypothesis can explain se appearances when it is acknowledged that all the prior ones re broken down. If we for the sake of the greatest simplicity nsider a swarm of meteorites at rest, and then assume that others m without approach it from all directions, their previous paths ing deflected, the question arises whether there will not be at some tance from the centre of the swarm a region in which collisions ll be most valid. If we can answer this question in the affirmative, will follow that some of the meteorites arrested here will begin to ve in almost circular orbits round the common centre of gravity.

The major axes of these orbits may be assumed to be not very rerse, and we may further assume that, to begin with, one set will eponderate over the rest. Their elliptic paths may throw the peri-ron passage to a considerable distance from the common centre of avity; and if we assume that the meteorites with this common an distance are moving in all planes, and that some are direct and me retrograde, there will be a shell in which more collisions will ce place than elsewhere. *Now, this collision surface will be practically only thing visible, and will present to us the exact and hitherto unex-ined appearance of a planetary nebula—a body of the same intensity luminosity at its edge and centre—thus putting on an almost phos-orescent appearance.*

If the collision region has any great thickness, the centre should pear dimmer than the portion nearer the edge.

Such a collision surface, as I use the term, is presented to us during meteoric display by the upper part of our atmosphere.

I append a diagram, Fig. 1, which shows how, if we thus sume movement round a common centre of gravity in a mass of eteorites, one of the *conditions of* movement being that the peri-*tron distance shall be somewhat* considerable, the mechanism which *duces the appearance of a* planetary nebula is at once made appa-

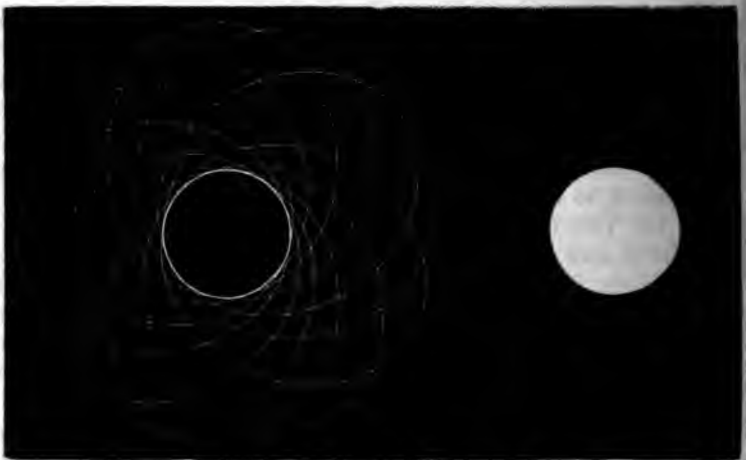


Fig. 1.—Suggested origin of the appearance presented by a planetary nebula. The luminosity is due to the collisions occurring along the sphere of intersection of the elliptic orbits of the meteorites. The left-hand diagram is a cross-section of the meteoric system, and the right-hand one shows the appearance of the collision-shell as seen from a point outside.

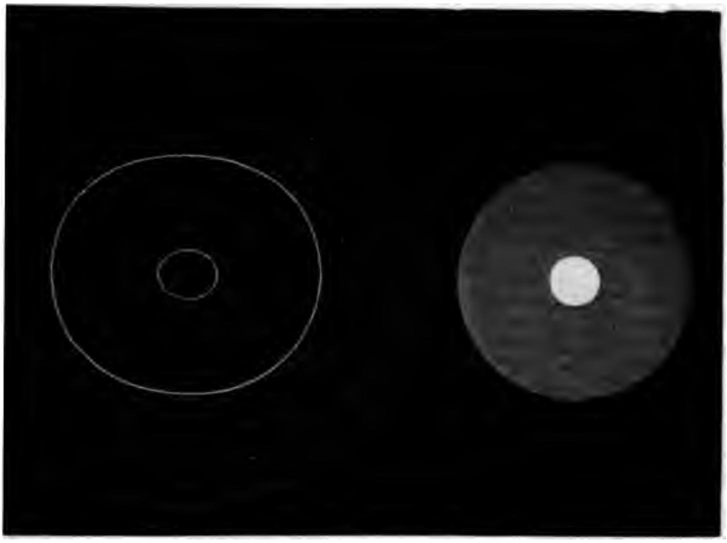
rent. The diagram shows the appearance on the supposition that the conditions of all the orbits with reference to the major axis shall be nearly identical, but the appearances would not be very greatly altered if we take the more probable case in which there will be plus and minus values.

*Globular Nebulæ showing Condensation until finally a Nebulous Star is reached.*

If we grant the initial condition of the formation of a collision-shell, we can not only explain the appearances put on by planetary nebulæ, but a continuation of the same line of thought readily explains those various other classes to which Herschel has referred, in which condensations are brought about, either by a gradual condensation towards the centre, or by what may be termed successive jumps. These condensations doubtless are among the earliest stages of nebular development.

To explain these forms we have only to consider what will happen to the meteorites which undergo collision in the first shell. They will necessarily start in new orbits, and it is suggested that an interior collision-shell will in this way be formed.

In consequence of the collisions the orbits will have a tendency to *get more and more elliptic*, while the pericentric distance will at the *same time be reduced;* the swarm will, in consequence of this action



-Suggestion as to the origin of a globular nebula with a brighter central tion. As in the former case, the luminosity of the fainter portion is due to collisions which occur along the sphere of intersection represented by the ger circle. After collision the meteorites will travel in new orbits, and there ll be an additional sphere of intersection, represented by the smaller circle. e left-hand diagram is a cross-section, and the right-hand one represents the pearance of the two collision-shells as seen from a point outside.

ally brighten towards the centre through collisions being possible r the centre, and ultimately we shall have nebulæ with a distinct us, the nucleus then representing the *locus* of most collisions. brightness may be sudden in certain spherical surfaces, or quite ial, according to the collision conditions in each swarm.
e final stage will be the formation of a nebulous star.

*Effects of Subsequent Rotation.—Spheroidal Nebulæ.*

such meteor-swarms as those we have considered, it must be that ion is, sooner or later, set up. Otherwise it would be impossible count for the spheroidal nebulæ at all. I am aware that in on's opinion the cause of this rotation was not mechanical, but moment we assume a meteoric origin of these globular clusters it aining the facts to assume that the intake will be exactly the at all points, and the moment the bombardment is more or less sed, rotation must follow sooner or later. Sir William Herschel, s paper of 1811 (p. 319), *says*, "If we consider this matter in a *al light, it appears that every* figure which is not already globu- *st have eccentric* nebulous matter, which, in its endeavour

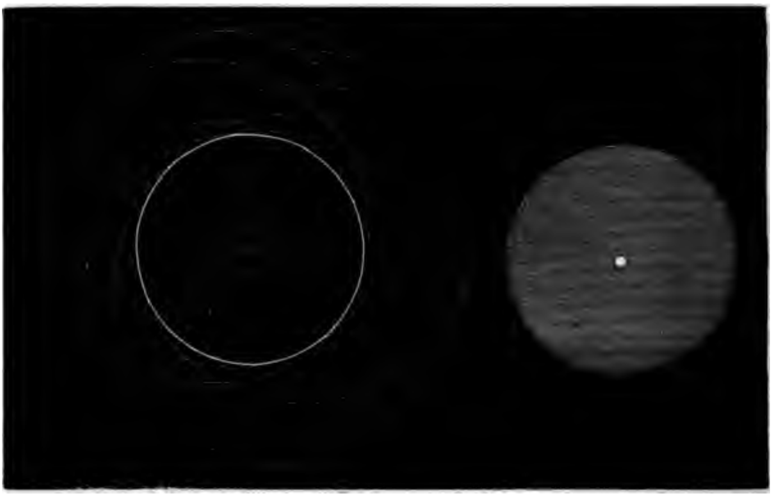


FIG. 3.—Suggestion as to the origin of a nebulous star. The orbits of the inner set of meteorites are very elliptic, so that the shell of intersection appears almost as a point. As in the previous cases, the left-hand diagram represents the meteoric systems in section, and the right-hand one the appearance from a point outside.

to come to the centre, will either dislodge some nebulosity which is already deposited, or slide upon it sideways, and in both cases produce a circular motion; so that, in fact, we can hardly suppose a possible production of a globular form without a subsequent revolution of nebulous matter, which in the end may settle in a regular rotation about some fixed axis."

Given, then, a globular swarm with a rotation around an axis, we have to discuss the phenomena produced by collisions under a new set of circumstances.

Here at once we have to account for the fact that the nearly spherical forms are very short-lived, for they are very rare; we seem to jump, as it were, from globes to very extended spheroids.

If it be conceded that from the above considerations we are justified in supposing that the elliptic and other spheroidal nebulæ really represent a higher stage of evolution than those presented to us by the globular form, it is clear that on the meteoritic hypothesis the greater part of the phenomena will represent to us what happens to such a system under the condition of a continuous bombardment of meteorites from without.

So soon as we have a minor axis, there will at first be most collisions *caused* by the movements of meteors, the paths of which are *most nearly parallel to it*; the result of this will be that the equatorial *plane will be intensified*, and then, later on, if we conceive the system

as a very extended spheroid, it is obvious that meteorites approaching it in directions parallel to its minor axis will have fewer chances of collisions than those which approach it, from whatever azimuth, in what we may term the equatorial plane. These evidently, at all events if they enter the system in any quantity, will do for the equatorial plane exactly what their fellows were supposed to do for the section in fig. 1, and we shall have on the general background of the symmetrically rotating nebula, which may almost be invisible in consequence of its constituent meteorites all travelling the same way and with nearly equal velocities, curves indicating the regions along which the entrance of the new swarm is interfering with the movements of the old one; if they enter in excess from any direction, we shall have broken rings or spirals.

This was suggested in my last paper. Various segments of rings will indicate the regions where most collisions are possible, and the absence of luminosity in the centre by no means demonstrates the absence of meteorites there.

Researches by Lord Rosse and others have given us forms of nebulæ which may be termed sigmoid and Saturnine, and these suggest that they and the elliptical nebulæ themselves are really produced by the rotation of what was at first a globular rotating swarm of meteorites, and that in these later revelations we pick up those forms which are produced by the continued flattening of the sphere into a spheroid under the meteoric conditions stated. It is worthy of remark that all the forms taken on by the so-called elliptic nebulæ described by the two Herschels, and by the spiral, sigmoid, and Saturnine forms which have been added to them by the labours of Lord Rosse and others, are recalled in the most striking manner by the ball of oil in Plateau's experiment, when rotations of different velocities are imparted to it.

The Saturnine form may, indeed, in some cases represent either the first or last stages in this period of the evolutionary process. I say *may* represent, in consequence of the extreme difficulty in making the observations so that in the early stages a spherical nebula, beginning to change into a spheroid, may have its real spheroidal figure cloaked by various conditions of illumination.

The true Saturnine form must, as in the case of Saturn itself, represent one of the latest forms in the meteor-swarm, because, if it be not continually fed from without, collisions must sooner or later bring all the members of the swarm to the centre of figure.

*Cometic Nebulæ.*

I do not know *that any explanation* has, so far, been suggested as *to the origin of these curious forms*, which were first figured by *Sir William Herschel, and of which* a number have recently been

observed in the southern hemisphere ('Observations of the Southern Nebulæ, made with the Great Melbourne Telescope,' Part I). It is clear that in them the conditions are widely different from those hitherto considered in this paper. I think that the meteoritic hypothesis satisfactorily explains them, on the supposition that we have either a very condensed swarm moving at a very high velocity through a sheet of meteorites at rest, or the swarm at rest surrounded by a sheet all moving in the same direction. It is a question of relative velocity.

If we consider the former case, it is clear that the collision region will be in the rear of the swarm, that the collisions will be due to the convergence of the members of the sheet due to the gravity of the swarm, and that the collision region will spread out like a fan behind the swarm.

The angle of the fan, and the distance to which the collisions are valid, will depend upon the velocity of the condensed swarm.

[Received March 26, 1888.]

## II. *Stars with Bright Lines or Flutings.*

I pointed out in my last paper that those stars in the spectra of which bright lines had been observed were in all probability the first result of nebulous condensation, both their continuous spectrum and that of the surrounding vapour being produced by a slightly higher temperature than that observed in nebulæ in which similar though not identical phenomena are observed.

I have recently continued my inquiries on this point; and I may say that all I have recently learned has confirmed the conclusions I drew in my last paper, while many of the difficulties have disappeared. Before I refer to these inquiries, however, it is necessary to clear the ground by referring to the old view regarding the origin of bright lines in stellar spectra, and to the question of hydrogen.

### *Reference to the Old View by which it was supposed some of the Bright-line Phenomena might be accounted for.*

In the views which, some years ago, were advanced by myself and others, to account for the bright lines seen in some of the "stars" to which reference has been made, the analogy on which they were based was founded on solar phenomena; the "stars" in question being supposed to be represented in structure by our central luminary. The main constituent of the solar atmosphere outside the photosphere is *hydrogen,* and it was precisely this substance which was chiefly revealed by these stellar observations and in the Novas, in which cases it was sometimes predominant. A tremendous development of

an atmosphere like that of the sun seemed to supply the explanation of the phenomena.

Acting on this view in 1878,* I attempted to catch these chromospheric lines in *α* Lyræ, abandoning the use of a cylindrical lens in front of the slit with this object in view.

Further, it was quite clear that if such gigantic supraphotospheric atmospheres existed, their bright lines might much modify their real absorption spectra; even "worlds without hydrogen" might be thus explained without supposing a *lusus naturæ*, and so I explained them.

That this view is untenable, as I now believe, and that it is unnecessary, will, I think, be seen from what follows. A long series of newly described phenomena, which are absolutely incomprehensible while it is applied to them, find, I think, a simple and sufficient explanation. I must hold that the view is untenable, because how a body constituted in any way like the sun could change its magnitude from the thirteenth to the sixth every year or so, or change its hydrogen lines from bright to dark once a week, passes comprehension; and the more closely a "star" resembles the sun the less likely are such changes to happen. Even the minor evolutionary changes are inexplicable on this hypothesis, chiefly because in a completely condensed mass the temperature must be very high and constant, while I have shown that the spectroscopic phenomena are those of a specially low temperature; and I may now add that many of the objects are extremely variable in the quantity and quality of the light they emit.

Another cause of the appearance of the hydrogen lines has been suggested by Mr. Johnstone Stoney ('Roy. Soc. Proc.,' vol. 17, p. 54). He considers it due to the clashing together of the atmospheres of two

* ". . . The sun which we see, the sun which sends us the majority of the light we receive, is but a small kernel in a gigantic nut, so that the diameter of the real sun may be, say, 2,000,000 miles. Suppose then that some stars have very large coronal atmospheres; if the area of the coronal atmosphere is small compared with the area of the section of the true disk of the sun, of course we shall get an ordinary spectrum of the star; that is to say, we shall get the indications of absorption which make us class the stars apart; we shall get a continuous spectrum barred by dark lines. But suppose that the area of the coronal atmosphere is something very considerable indeed, let us assume that it has an area, say fifty times greater than the section of the kernel of the star itself; now, although each unit of surface of that coronal atmosphere may be much less luminous than an equal unit of surface of the true star at the centre, yet, if the area be very large, the spectroscopic writing of that large area will become visible side by side with the dark lines due to the brilliant region in the centre where we can study absorption; other lines (bright ones) proceeding from the exterior portion of that star will be visible in the spectrum of the apparent *point* we call a star. Now it is difficult to say whether such a body *as that is a star or a nebula.* We may look upon it as a nebula in a *certain stage of condensation; we* may look upon it as a star at a certain stage of *growth."—'Roy. Soc. Proc.,' vol. 27, 1876,* p. 50.

stars, the outer constituent of the atmosphere—hydrogen—alone being raised by the friction to brilliant incandescence.

Another objection we can urge against the old view is that all bodies in the universe cannot be finished suns in the ordinary sense, and that it leaves out of account all possible processes of manufacture, not only of single stars, but of double and multiple systems, at all stages between nebula and sun; while the new one, by simply changing the unit from the star to each individual constituent, it is hardly too much to say, explains everything, though it is perfectly true that in some of the steps a considerable acquaintance with spectroscopic phenomena is necessary to realise the beauty and the stringency of the solutions.

*The Question of Hydrogen in the Case of Bright-line Stars.*

It may be convenient also that I should summarise the various conditions under which the lines of hydrogen are observed in the meteoritic swarms we are now considering.

In the "nebulæ" we begin with the widest interspaces. Future investigation may, as I have suggested, show that those in which the hydrogen lines are absent are the most widely spaced of all. Be this as it may, it is a matter of common knowledge that in the brighter nebulæ, such as that of Orion, to take an instance, we have hydrogen associated with the low-temperature radiation of olivine. That the hydrogen is electrically excited to produce this glow is proved by the fact that the temperature of the meteorites themselves must be very low; otherwise the magnesium would not show itself without the manganese and iron constituents, and the continuous spectrum would be much brighter and longer than it is.

In the former paper I showed that in my laboratory experiments, when the pressure was slightly increased in a tube containing gases obtained from meteorites, the carbon bands began to be visible. We should expect this to happen therefore in a meteor swarm at some point at which the mean interstitial space was smaller than that accompanied by the appearance of the hydrogen lines; and it would be natural that both should be seen together at an early stage and both feeble, by which I mean not strongly developed, as hydrogen is not strongly developed even in the nebula of Orion, none of the ultra-violet lines being visible in a photograph, while the magnesium line is.

The association of the low-temperature lines of hydrogen with the flutings of carbon is therefore to be expected, and I shall subsequently show that we have such an association in the so-called bright-line stars; and even at a further stage of development, in stars like α Orionis, the hydrogen is still associated with the carbon.

*The Cometic Nature of Stars with Bright Lines in their Spectra.*

Seeing that the hypothesis I am working on demands that the luminosity in stars and the bright lines in their spectra are produced by the collisions of meteorites, the spectra of those bodies must in part resemble those of comets, in which bodies by common consent the luminosity is now acknowledged to be produced by collisions of meteorites.

We must, however, consider the vast difference in the way in which the phenomena of distant and near meteoric groups are necessarily presented to us; and, further, we must bear in mind that in the case of comets, however it may arise, there is an action which drives the vapours produced by impacts outward from the swarm in a direction opposite to that of the sun.

It must be a very small comet which, when examined spectroscopically in the usual manner, does not in consequence of the size of the image on the slit enable us to differentiate between the spectra of the nucleus and envelopes. The spectrum of the latter is usually so obvious, and the importance of observing it so great, that the details of the continuous spectrum of the nucleus, however bright it may be, are almost overlooked.

A moment's consideration, however, will show that if the same comet were so far away that its whole image would be reduced to a point on the slit-plate of the instrument, the differentiation of the spectra would be lost; we should have an integrated spectrum in which the brightest edges of the carbon bands, or some of them, would or would not be seen superposed on a continuous spectrum.

The conditions of observation of comets and stars being so different, any comparison is really very difficult; but the best way of proceeding is to begin with the spectrum of comets, in which, in most cases, for the reason given, the phenomena are much more easily and accurately recorded.

But even in the nucleus of a comet as in a star it is much more easy to be certain of the existence of bright lines than to record their exact positions,* and as a matter of fact bright lines, including in all probability hydrogen, have been recorded, notably in Comet Wells and in the great comet of 1882.

The main conclusion to which my researches have led me is that the stars now under consideration are almost identical in constitution with comets between that condition in which, as in those of 1866 and 1867, they give us the absolute spectrum of a nebula and that put on by the great comet of 1882.

* "*Observations of Comet III,* 1881, *June* 25.—The spectrum of the nucleus is continuous; *that of the coma shows the* usual bands. With a narrow slit there are *indications of many lines just beyond* the verge of distinct visibility."—Copeland, '*Copernicus,*' *vol. 2, p. 226.*

I am aware that this conclusion is a startling one, but a little consideration will show its high probability, and a summary of all the facts proves it, I think, beyond all question.

While we have bright lines in comets, it can be shown that some of them are the remnants of flutings. Thus in Comet III of 1881, as the carbon lines died away the chief manganese fluting at 558 became conspicuously visible; it had really been recorded before then. The individual observations are being mapped in order that the exact facts may be shown. It may probably be asked how it happened that the fluting of magnesium at 500 was not also visible. Its absence, however, can be accounted for: it was *masked* by the brightest carbon fluting at 517, whereas the carbon fluting which under other circumstances might mask the manganese fluting at 558 is always among the last to appear very bright and the first to disappear.

In the great comet of 1882, which was most carefully mapped by Copeland, very many lines were seen, and indeed many were recorded, and it looks as if a complete study of this map will put us in possession of many of the lines recorded by Sherman in the spectrum of γ Cassiopeiæ. We have then three marked species of non-revolving swarms going on all fours with three marked species of revolving ones, and in this we have an additional argument for the fact that the absence in the former of certain flutings which we should expect to find may be attributed to masking by the carbon flutings.

We have next, then, to show that there are carbon bands in the bright-line stars.

There is evidence of this. Among the bright lines recorded is the brightest carbon fluting at 517. This is associated with those lines of magnesium and manganese and iron visible at a low temperature which have been seen in comets.

But we have still more evidence of the existence of carbon. In a whole group of bright-line stars there is a bright band recorded at about 470, while, less refrangible than it, there appears a broad absorption band. I regard it as extremely probable that we have here the bright carbon band 467—474, and that the appearance of an absorption band is due to the fact that the continuous spectrum of the meteorites extends only a short distance into the blue.

If we consider such a body as Wells's comet, or the great comet of 1882, at so great a distance from us that only an integrated spectrum would reach us, in these cases the spectrum would appear to extend very far, and more or less continuously, into the blue; but this appearance would be brought about, not by the continuous spectra of the meteorites themselves, but by the addition of the hydrocarbon fluting at 431 to the other hot and cold carbon bands in that part of the spectrum.

There are other grounds which may be brought forward to suggest

that the difference between comets and the stars now under discussion is more instrumental than physical.

Supposing that the cometic nature of these bodies be conceded, laboratory work will eventually show us which flutings and lines will be added to the nebula spectrum upon each rise of temperature.

The difficulties of the stellar observations must always be borne in mind. It will also be abundantly clear that a bright fluting added to a continuous spectrum may produce the idea of a bright line at the sharpest edge to one observer, while to another the same edge will appear to be preceded by an absorption band.

### III. *Stars with Bright Flutings accompanied by Dark Flutings.*

I also showed in the paper to which reference has been made that the so-called "stars" of Class III*a* of Vogel's classification are not masses of vapour like our sun, but really swarms of meteorites; the spectrum being a compound one, due to the radiation of vapour in the interspaces and the absorption of the light of the red- or white-hot meteorites by vapours volatilised out of them by the heat produced by collisions. The radiation is that of carbon vapour, and some of the absorption, I stated, was produced by the chief flutings of manganese.

These conclusions were arrived at by comparing the wave-lengths of the details of spectra recorded in my former paper with those of the bands given by Dunér in his admirable observations on these bodies.*

The discovery of the cometic nature of the bright-line stars greatly strengthens the view I then put forward, not only with regard to the presence of the bright flutings of carbon, but with regard to the actual chemical substances driven into vapour. From the planetary nebulæ there is an undoubted orderly sequence of phenomena through the bright-line stars to those now under consideration, if successive stages of condensation are conceded.

I shall return to these bodies at a later part of this memoir.

### IV. *Stars in which Absorption Phenomena predominate.*

I do not suppose that there will be any difficulty in recognising, that if the nebulæ, stars with bright lines, and stars of the present Class III*a* are constituted as I state them, all the bodies more closely resembling the sun in structure, as well as those more cooled down, must find places on a temperature curve pretty much as I have placed

* *"Les Étoiles à Spectres de la troisième classe."*—'Kongl. Svenska Vetenskaps-*Akademiens Handlingar,' Band 21, No. 2,* 1885.

them; the origin of these groups being, first still further condensation, then the condition of maximum temperature, and finally the formation of a photosphere and crust.

We shall be in a better position to discuss these later stages when the classifications hitherto suggested have been considered.

## Part II.—Classification into Groups.

### I. Former Classifications of Stars.

In the various classifications of the celestial bodies which have been attempted from time to time, nebulæ and comets have been regarded as things apart from the stars; but from what I have stated in the first part of this paper, relating to the origin of the various groups of heavenly bodies, it is clear that it is not only unnecessary but unphilosophical to make such a distinction; and, indeed, if any such separation were needed, such a result would seem to indicate that the line of evolution is by no means so simple and clear as it really seems to be. But although it is no longer necessary to draw this distinction, it is important that I should state the various spectroscopic classifications which have been attempted in the case of the stars. With this information before us, we shall be better able to see the definite lines on which any new classification must be based to include all celestial forms.

#### *Fraunhofer, Rutherfurd, and Secchi.*

When we inquire into the various labours upon which our present knowledge of the spectra of the various orders of "stars" is based, the first we come across are those of Fraunhofer, who may be said to have founded this branch of scientific inquiry in the year 1814.

Fraunhofer not only instituted the method of work which now is found to be the most effective, but his observations at that time were so excellent that he had no difficulty in finding coincidences between lines in the spectrum of the sun and of Venus.

Fraunhofer's reference in his observations runs as follows:—

"I have also made several observations on some of the brightest fixed stars. As their light was much fainter than that of Venus, the brightness of their spectrum was consequently still less. I have nevertheless seen, without any illusion, in the spectrum of the light of Sirius, three large lines, which apparently have no resemblance with those of the sun's light. One of them is in the green, and two in the blue space. Lines are also seen in the spectrum of other fixed stars of the first magnitude; but these stars appear to be different from one another in relation to these lines. As the object-glass of the telescope of the theodolite has only thirteen lines of aperture, these

experiments may be repeated, with greater precision, by means of an object-glass of greater dimensions."*

He did not attempt to classify his observations on stellar spectra, but, as pointed out by Professor Dunér ("Sur les Étoiles à Spectres de la Troisième Classe," p. 3), those that he most particularly mentions are really remarkably diverse in their characteristics.

In these researches Fraunhofer was followed by Rutherfurd, who, in the year 1863, was the first to indicate that the various stellar spectra which he had then observed were susceptible of being arranged into different groups. His paper was published in 'Silliman's Journal' (vol. 35, p. 71), and, after giving an account of the observations actually made, continues as follows:—

"The star spectra present such varieties that it is difficult to point out any mode of classification. For the present, I divide them into three groups:—First, those having many lines and bands, and mostly resembling the sun, viz., Capella, $\beta$ Geminorum, $\alpha$ Orionis, Aldebaran, $\eta$ Leonis, Arcturus, and $\beta$ Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the sun, and are white stars. The third group, comprising $\alpha$ Virginis, Rigel, &c., are also white stars, but show no lines; perhaps they contain no mineral substance, or are incandescent without flame."

Soon afterwards Secchi carried on the inquiry, and began in 1865 by dividing the objects he had then observed into two types. These two types were subsequently expanded in 1867 into three ('Catalogo delle Stelle di cui si è determinato lo Spettro Luminoso,' Secchi, Parigi, 1867): first, white stars, like $\alpha$ Lyræ; secondly, yellow stars, like Arcturus; and thirdly, deeply coloured stars, like $\alpha$ Herculis and $\alpha$ Orionis. The order of these types was not always as stated, but I have not been able to find the exact date at which the order was changed (Dunér, "Sur les Étoiles," p. 128). Secchi subsequently added a fourth type, in which the flutings were less numerous. There is little doubt that Secchi was led to these types not so much by any considerations relating to the chemical constitution of the atmospheres of these bodies, as in relation to their colours. His first classifications, in fact, simply separated the white stars from the coloured ones (see on this point 'Le Scopirte Spettroscopiche,' A. Secchi, Roma, 1865).

The fourth type included, therefore, stars of a deeper red colour than those of the third, and Secchi pointed out that this change of colour was accompanied by a remarkable change in the spectrum; in fact, of Secchi's four types thus established, the first and second had

* "*On the Refractive and Dispersive Power of Different Species of Glass, with an Account of the Lines which cross the Spectrum.*"—Fraunhofer, translated in '*Edinburgh Philosophical Journal*,' *vol. 10, October* to April, 1823–24, p. 39.

line spectra and the third and fourth had fluted ones. At that ti: the important distinction to be drawn between line- and fluted-spec was not so well recognised as it is at present; and further the relati of spectra to temperature was not so fully considered. Secchi, a result of laboratory work, however, at once showed an undoubt connexion between the absorption flutings in the stars of the fou type and the bright ones seen in the spectrum or carbon under certa conditions; and although this conclusion has been denied, it has sin been abundantly confirmed by Vogel and others (see Vogel, 'Publi tionen, &c., Potsdam,' No. 14, 1884, p. 31).

*Relation to Temperature.*

At the time that Secchi was thus classifying the stars, the quest was taken up also by Zöllner, who in 1865 first threw out the sugg tion that the spectra might probably enable us to determine som what as to the relative ages of these bodies; and he suggested tl the yellow and red light of certain stars were indications of a red tion of temperature (Zöllner, 'Photometrische Untersuchung p. 243).

In 1868 this subject occupied the attention of Ångström w special reference to the contrasted spectra of lines and flutings. this he wrote as follows, showing that temperature considerati might help us in the matter of variable stars ('Recherches sur Spectre solaire,' Upsala, 1868):—

"D'après les observations faites par MM. Secchi et Huggins, raies d'absorption dans les spectres stellaires sont de deux espèc chez l'une, le spectre est rayé de lignes très-fines, comme le spec solaire; chez l'autre, les raies constituent des groupes entiers à espa égaux ou des bandes nuancées. Ces derniers groupes appartienn vraisemblablement aux corps composés, et je mentionnerai, en p ticulier, que ceux trouvés dans le spectre de α Orionis ressembl fort aux bandes lumineuses que donne le spectre de l'oxyde de m ganèse. Supposé que ma théorie soit juste, l'apparition de ces ban doit donc indiquer que la température de l'étoile est devenue as basse pour que de telles combinaisons chimiques puissent se form et se conserver.

"Entre ces deux limites de température chez les étoiles, limites q l'on peut caractériser par la présence de l'une ou de l'autre esp des raies d'absorption, on peut s'imaginer aussi un état intermédia dans lequel les gaz composés peuvent se former ou se dissocier, suiv les variations de température auxquelles ils sont assujettis par l'act chimique même. Dans cette classe doivent probablement être co *prises* les étoiles dont l'intensité de lumière varie plus ou mo *rapidement, et avec une* périodicité plus ou moins constante."

*In the year 1873, I* referred to this subject in my Bakerian Lec

('Phil. Trans.,' vol. 164, 1874, p. 492), in which I attempted to bring to bear some results obtained in solar inquiries upon the question of stellar temperatures.

I quote the following paragraphs:—

I. The absorption of some elementary and compound gases is limited to the most refrangible part of the spectrum when the gases are rare, and creeps gradually into the visible violet part, and finally to the red end of the spectrum, as the pressure is increased.

II. Both the general and selective absorption of the photospheric light are greater (and therefore the temperature of the photosphere of the sun is higher) than has been supposed.

III. The lines of compounds of a metal and iodine, bromine, &c., are observed generally in the red end of the spectrum, and this holds good for absorption in the case of aqueous vapour.

Such spectra, like those of the metalloids, are separated spectroscopically from those of the metallic elements by their columnar or banded structure.

IV. There are, in all probability, no compounds ordinarily present in the sun's reversing layer.

V. When a metallic compound vapour, such as is referred to in III, is dissociated by the spark, the band spectrum dies out, and the elemental lines come in, according to the degree of temperature employed.

Again, although our knowledge of the spectra of stars is lamentably incomplete, I gather the following facts from the work already accomplished with marvellous skill and industry by Secchi, of Rome.

VI. The sun, so far as the spectrum goes, may be regarded as a representative of class ($\beta$) intermediate between stars ($\alpha$) with much simpler spectra of the same kind, and stars ($\gamma$) with much more complex spectra of a different kind.

VII. Sirius, as a type of $\alpha$, is (1) the brightest (and therefore hottest?) star in our northern sky; (2) the blue end of its spectrum is open,—it is only certainly known to contain hydrogen, the other metallic lines being exceedingly thin, thus indicating a small proportion of metallic vapours; while (3) *the hydrogen lines in this star are enormously distended*, showing that the chromosphere is largely composed of that element.

There are other bright stars of this class.

VIII. As types of $\gamma$ the red stars may be quoted, the spectra of which are composed of channelled spaces and bands, and in which naturally the blue end is closed. Hence the reversing layers of these stars probably contain metalloids, or compounds, or both, in great quantity; and in their *spectra not only* is hydrogen absent, but the *metallic lines are reduced in thickness* and intensity, which in the *light of V., ante, may indicate that* the metallic vapours are being

*associated.* It is fair to assume that these stars are of a lower temperature than our sun.

In the same year, in a letter to M. Dumas, published in the 'Comptes Rendus,'* I again pointed out that, if we consider merely the scale of temperature, a celestial body with flutings in its spectrum would be cooler than one which had lines in its spectrum; and I also pointed out that, taking the considerable development of the blue end of the spectrum in white stars as contrasted with its feeble exhibition in stars like our sun, we had strong presumptive evidence to the effect that the stars like $\alpha$ Lyræ, with few lines in their spectra, were hotter than those resembling our sun, in which the number of lines was very much more considerable, and I added an inference from this: "plus une étoile est chaude, plus son spectre est simple." This related merely, as I have said before, to the consideration of one line of temperature.

### *Vogel's Classification.*

In the year following my paper, the most considerable classification which has been put forward of late years was published by Dr. Vogel ('Astr. Nachr.,' No. 2000), who, basing his work on the previous types of Secchi, and also taking into account the inference I drew in my letter to Dumas, modified Secchi's types to a certain extent, but always along one line of temperature, the leading idea being, as I gather from many remarks made in Dunér's admirable memoir, to be referred to presently, that the classification is based upon descending temperatures, and that all the stars included in it are supposed at one time or other to *have had* a spectrum similar to that of $\alpha$-Lyræ.†

This classification is as follows:—

* "Il semble que plus une étoile est chaude, plus son spectre est simple et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques. Ainsi nous avons:—

"(1) Des étoiles très brillantes, où nous ne voyons que l'hydrogène *en quantité énorme*, et le magnésium.

"(2) Des étoiles plus froides, comme notre soleil, où nous trovons:—

H + Mg + Na.
H + Mg + Na + Ca, Fe, &c.;

dans ces étoiles, pas de métalloïdes.

"(3) Des étoiles plus froides encore, dans lesquelles tous les éléments métalliques sont associés, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des métalloïdes et des composés.

"(4) Plus une étoile est âgée, plus l'hydrogène libre disparaît; sur la terre, nous *ne trouvons plus* l'hydrogène en liberté."

† *"Car selon la théorie il* faudra que tôt ou tard toutes les étoiles de la première *classe deviennent de la seconde,* et celles-ci de la troisième."—(Dunér.)

CLASS I. *Spectra in which the Metallic Lines are extremely Faint or entirely Invisible.*—The most refrangible parts, blue and violet, are very vivid. The stars are white.

(*a.*) Spectra in which the lines of hydrogen are very strong.

(*b.*) Spectra in which the lines of hydrogen are wanting.

(*c.*) Spectra in which the lines of hydrogen and $D_3$ are bright.

CLASS II. *Spectra in which the Metallic Lines are Numerous and very Visible.*—The blue and violet are relatively weaker; in the red part there are sometimes faint bands. The colour of the star is clear bluish-white to deep reddish-yellow.

(*a.*) Spectra with numerous metallic lines, especially in the yellow and green. The lines of hydrogen are generally strong, but never as strong as in the stars of Class I. In some stars they are invisible, and then faint bands are generally seen in the red formed by very close lines.

(*b.*) Spectra in which besides dark lines and isolated bands there are several bright lines.

CLASS III. *Spectra in which besides the Metallic Lines there are numerous Dark Bands in all parts of the Spectrum, and the Blue and Violet are remarkably Faint.*—The stars are orange or red.

(*a.*) The dark bands are fainter towards the red.

(*b.*) The bands are very wide, and the principal are fainter towards the violet.

It is pointed out that if this classification be true, there must be links between all the classes given. Now it is perfectly obvious that if this classification includes in its view all the stars, and if there is a line of ascending as well as descending temperatures—that is to say, if some of the stars are increasing their temperatures, while others are diminishing them—the classification must give way.

It is not difficult to see, in the light of my communication to the Society of November 17th, that it has given way altogether, and principally on this wise.

The idea which underlies the classification is that a star of Class I on cooling becomes a star of Class II, and that a star of Class II has as it were a choice before it of passing to Class III*a* or Class III*b*. Thus under certain conditions its spectrum will take on the appearance of Secchi's third type, Class III*a* (Vogel); on certain other conditions it will take on the appearance of Secchi's fourth type, Class III*b* (Vogel). There is now, however, no doubt whatever that Secchi's Class III*a* represents stars in which the temperature is increasing, and with conditions not unlike those of the nebulæ—that is to say, the meteorites are discrete, and are on their way to form bodies *of Class II and* Class I by the ultimate vaporisation *of all their meteoric constituents.* There is also no doubt that th *stars included in Class IIIb* have had their day; that their temper

ture has been running down, until owing to reduction of temperature they are on the verge of invisibility brought about by the enormous absorption of carbon in their atmospheres.

Pechüle was the first to object to Vogel's classification, mainly on the ground that Secchi's types 3 and 4 had been improperly brought together; and my work has shown how very just his objection was, and how clear-sighted was his view as to the true position of stars of Class III*b*. I give the following extract from his memoir:—

"M. Vogel a proposé une classification suivant les diverses phases de refroidissement indiquées par les spectres, dans laquelle il fait des types III et IV de Secchi deux subdivisions d'une même classe, III*a* et III*b*. Mais je trouve certaines difficultés negatives contre cette classification relativement au rôle qu'y joue le III*b*. En effet, il est admis que le IV type de Secchi se distingue nettement du III type, non seulement par la position et la quantité des zones obscures, mais aussi par le fait très-remarquable, que les principales de ces zones sont bien définies et brusquement interrompues du côté du violette dans le III type, du côté du rouge dans le IV. Or, si le IV type doit représenter une des phases de refroidissement, par lesquelles passent les étoiles, on peut faire deux hypothèses. La première est que le spectre du IV type soit co-ordonné au spectre du III type, de manière qu'il ait des étoiles, qui passent de la phase représentée par le II type, à la phase représentée par le III type, et d'autres, qui passent directement du II type au IV. Mais cette hypothèse est inadmissible. Car on connaît de spectres entremédiaires entre le I et le II type, et entre le II et III; mais on ne connaît pas, à ce que je sache, de spectres du II type tendant au IV. Reste donc l'hypothèse, que la phase de refroidissement, représentée par le spectre du IV type, soit postérieure à la phase représentée par le III type, de manière que les spectres des étoiles passent du III au IV type. Si ce passage se fait peu à peu, il devrait avoir des spectres entremédiaires entre le III et le IV type; mais quoique Secchi par exemple le 17 Jan., 1868, ait déterminé le spectre de l'étoile 273 Schjell., comme semblant entremédiaire entre le III et le IV type, il l'a plus tard reconnu du IV type, et l'existence de spectres de III—IV type n'est nullement prouvée. On pourrait objecter que les étoiles du IV type sont peu nombreuses et en général si petites que leurs spectres sont difficiles à voir, et que par conséquent il pourrait y avoir parmi ces spectres quelques-uns, qui se rapprochassent du III type. Mais je réponds à cette remarque, que les spectres du III—IV type, indiquant une phase moins refroidie, devraient au contraire en général appartenir à des étoiles plus grandes que celles avant des spectres du IV type. Si on veut supposer que le passage du III au IV type se fasse subitement, ou par une catastrophe, pendant laquelle apparaissent des lignes brillantes, cettes supposition même

constituerait une différence physique bien plus distincte entre le III et le IV type qu' entre le II et le III; et le IV type représenterait une phase bien distincte, la dernière peut-être avant l'extinction totale. Le rôle physique du IV type est donc encore si mystérieux, que j'ai cru pouvoir encore me conformer à l'exemple de d'Arrest, en suivant la classification formelle de Secchi."—C. F. Pechüle, 'Expédition Danoise pour l'Observation du Passage de Vénus, 1882,' p. 25, (Copenhagen, J. H. Schultz, 1883).

## II. Proposed New Grouping of all Celestial Bodies according to Temperature.

Having, then, gone over the various classifications of stars according to their spectra, I now proceed to consider the question of the classification of celestial bodies from a more advanced point of view. I pointed out in the year 1886 that the time had arrived when stars with increasing temperatures would require to be fundamentally distinguished from those with decreasing temperatures, but I did not then know that this was so easy to accomplish as it now appears to be ('Nature,' vol. 34, p. 228); and, as I have already stated, when we consider the question of classification at all, it is neither necessary nor desirable that we should limit ourselves to the stars; we must include the nebulæ and comets as well. Stellar variability should not introduce any difficulties, seeing that as a rule in its extremest form it is the passage from one spectrum to another, even if of a different type, owing to sudden changes of temperature.

In the first classification on these lines, which is certain to be modified as our knowledge gets more exact, it is desirable to keep the groups as small in number as possible; the groups being subsequently broken up into sub-groups, or, even into species, as the various minute changes in spectra brought about by variations of temperature are better made out.

For the purpose of making clear what follows, I here introduce from my paper of November 17th, the "temperature curve," on which is shown the distribution of nebulæ, comets, and of stars as divided into classes by Vogel, on the two arms of the curve.

On one arm of this we have those stages in the various heavenly bodies in which in each case the temperature is increasing, while on the other arm we have that other condition in which we get first vaporous combination, and then ultimately the formation of a crust due to the gradual cooling of the mass, in dark bodies like, say, the companion to Sirius. At the top we of course have that condition in which the highest temperature must be assumed to exist.

To begin, then, a more general classification with the lowest temperatures, it is known that the nebulæ and comets are distinguished


FIG. 4.—Temperature curve, showing the relative temperatures of the different orders of celestial bodies. The top of the curve represents the highest temperatures, and the bottom of each arm the lowest. On the left arm, the temperatures are increasing, on the right they are decreasing. The diagram shows the relative temperatures of Vogel's classes.

from most stars by the fact that we get evidence of radiation alone, or almost alone so far as we know. Absorption has been suspected in the spectra of some nebulæ,* and has been observed beyond all doubt in some comets.† But there are some stars in which we also get radiation, accompanied by certain absorption phenomena. But there is no difficulty in showing that nebulæ and comets are more special on account of their bright lines than on account of their absorption bands. I have already shown that in all probability the stars with bright lines are most closely allied with nebulæ. Indeed, it seems as if they are very nearly akin to those condensations in nebulæ, showing an undoubted olivine and hydrogen spectrum, which gave them the appearance of resolvability. It seems, also, highly probable that future observations with instruments of great light-collecting power, will show that in nebulæ, the spectra of which are recorded as continuous, lines including the remnants of some of the carbon flutings, which there is good reason to believe have already been traced in the spectra of bright line stars, are also present. From this point of view, the various recorded observations of regions of different colour in certain nebulæ acquire an additional interest. It is also clear that since the only real difference between comets and other meteor swarms of equal denseness is that the former are in motion round the centre of our system, comets whether at aphelion or at perihelion will fall into this group. We may, therefore, form the first group of bodies which are distinguished by the presence of bright lines or flutings in the spectrum.

The great distinction between the first group and the second would be that evidences of absorption now become prominent, and side by side with the bright flutings of carbon and occasionally the lines of hydrogen we have well-developed fluting absorption.

The second group, therefore, is distinguished from the first by mixed flutings as well as lines in the spectrum.

* "Nebula [No. 117, 51 h. 32 M. R.A. 0 h. 35 m. 5·3 s.; N.P.D. 49° 54′ 12·7″. Very, very bright; large, round; pretty suddenly much brighter in the middle].—This small but bright companion of the great nebula in Andromeda presents a spectrum exactly similar to that of 31 M. [the great nebula in Andromeda]. The spectrum appears to end abruptly in the orange; and throughout its length is not uniform, but is evidently crossed either by lines of absorption or by bright lines."—(Huggins, 'Phil. Trans.,' vol. 154, p. 441.)

† "A dark band was noticed at wave-length 567·9."—(Copeland, "Comet III, 1881," 'Copernicus,' vol. 2, p. 226.)

"May 20.—With none of these dispersions could any bright bands, properly so-called, be distinguished; but two faint broad *dark* bands, or what gave that impression, crossed the spectrum. . . . A third dark band was suspected near D on the blue side of that line."—(Maunder, "Comet *a*, 1882 (Wells)," 'Greenwich *Spectroscopic Observations,' 1882, p. 34.)*

*"The dark bands were observed* again, and their wave-lengths measured on *May 31."—(Ibid., p. 35.)*

The passage from the second group to the third brings us to those bodies which are increasing their temperature, in which radiation and fluting absorption have given place to line absorption.

At present, the observations already accumulated have not been discussed in such a way as to enable us to state very definitely the exact retreat of the absorption—by which I mean the exact order in which the absorption lines fade out from the first members to the last in the group. We know generally that the earlier bodies will contain the line absorption of those substances of which we get a paramount fluting absorption in the prior group. We also know generally that the absorption of hydrogen will increase while the other diminishes.

The next group—the fourth, brings us to the stage of highes temperature, to stars like α Lyræ; and the division between thi group and the prior one must be more or less arbitrary, and cannot a present be defined. One thing, however, is quite clear, that n celestial body without all the ultra-violet lines of hydrogen discovere by Dr. Huggins can claim to belong to it.

We have now arrived at the culminating point of temperature, an now pass to the descending arm of the curve. The fifth group, there fore, will contain those bodies in which the hydrogen lines begin t decrease in intensity, and other absorptions to take place in conse quence of reduction of temperature.

One of the most interesting problems of the future will be to watc what happens in bodies along the descending scale, as compared wit what happens to the bodies in Group III, on the ascending one. Bu it seems fair to assume that physical and chemical combinations wi now have an opportunity of taking place, thereby changing the con stituents of the atmosphere; that at first with every decrease of tem perature an increase in the absorption lines may be expected, but i will be unlikely that the coolest bodies in this group will resemble th first one in Group III.

The next group, the sixth, is Secchi's type IV, and Vogel's Class III its distinct characteristics being the absorption flutings of carbon The species of which it will ultimately be composed are already appa rently shadowed forth in the map which accompanies Dunér's volume and they will evidently be subsequently differentiated by the gradua addition of other absorptions to that of carbon, while at the sam time the absorption of carbon gets less and less distinct.

To sum up, then, the classification I propose consists of the follow ing groups:—

Group I.—Radiation lines and flutings predominant. Absorptic beginning in the last species.

*Group II.—Mixed* radiation and absorption predominant.

*Group III.—Line* absorption predominant, with increasing te

perature. The various species will be marked by increasing simplicity of spectrum.

Group IV.—Simplest line absorption predominant.

Group V.—Line absorption predominant, with *decreasing* temperature. The various species will be marked by decreasing complexity of spectrum.

Group VI.—Carbon absorption predominant.

Group VII.—Extinction of luminosity.

It will be seen from the above grouping that there are several fundamental departures from previous classifications, especially that of Vogel.

The presence of the bright flutings of carbon associated with dark metallic flutings in the second group, and the presence of only absorbing carbon in the sixth, appears to be a matter of fundamental importance, and to entirely invalidate the view that both groups (the equivalents of III*a* and III*b* of Vogel) are produced from the same mass of matter on cooling.

This point has already been dwelt upon by Pechüle.

Another point of considerable variation is the separation of stars with small absorption into such widely different groups as the first and fourth, whereas Vogel classifies them together on the ground of the small absorption in the visible part of the spectrum. But that this classification is unsound is demonstrated by the fact that in these stars, such as $\gamma$ Cassiopeiæ and $\beta$ Lyræ, we have intense variability. We have bright hydrogen lines instead of inordinately thick dark ones; and on other grounds, which I shall take a subsequent opportunity of enlarging upon, it is clear that the physical conditions of these bodies must be as different as they pretty well can be.

It will be seen also that, with our present knowledge, it is very difficult to separate those stars the grouping of which is determined by line absorption into the Groups III and V, for the reason that so far, seeing that only one line of temperature, and that a descending one, has been considered, no efforts have been made to establish the necessary criteria. I noted this point in the paper to which I have already referred in connexion with the provisional curve.

## Part III.—Sub-Groups and Species of Group I.

### I. Sub-Group. Nebulæ.

Having, in the preceding part of this memoir, attempted to give a general idea of that grouping of celestial bodies which in my opinion best accords with *our present knowledge*, and which has been based *upon the assumed meteoric origin* of all of them, I now proceed to *test the hypothesis further by* showing how it bears the strain put

upon it when, in addition to furnishing us with a general grouping, it is used to indicate how the groups should be still further divided, and what specific differences may be expected.

The presence or absence of carbon will divide this group into two main sub-groups.

The first will contain those nebulæ in which only the spectrum of the meteoric constituents is observed with or without the spectrum of hydrogen added.

It will also contain those bodies in which the nebula spectrum gets almost masked by a continuous one, such as Comets 1866 and 1867, and the great nebula in Andromeda.

In the second sub-group will be more condensed swarms still, in which, one by one, new lines are added to the spectra, and carbon makes its appearance; while probably the last species in this sub-group would be bodies represented by $\gamma$ Cassiopeiæ.

*Species of Nebulæ.*

I have elsewhere referred to the extreme difficulty of spectroscopic discrimination in the case of the meteor swarms which are just passing from the first stage of condensation, and it may well be that we shall have to wait for many years before a true spectroscopic classification of the various aggregations which I have indicated, can be made.

It is clear from what has gone before that in each stage of evolution there will be very various surfaces and loci of collision in certain parts of all the swarms, and we have already seen that even in the nebulosities discovered by Sir Wm. Herschel, which represent possibly a very inchoate condition, there are bright portions here and there.

If the conditions are such in the highly elaborated swarms and in the nebulosities that the number of collisions in any region per cubic million miles is identical, the spectroscope will give us the same result. In the classification of the nebulæ, therefore, the spectroscope must cede to the telescope when the dynamical laws, which must influence the interior movements of meteoric swarms, have been fully worked out. The spectroscope, however, is certainly at one with the telescope in pointing out that the so-called planetary nebulæ are among the very earliest forms—those in which the collisions are most restricted in the colliding regions. The colour of these bodies is blue tinged with green; they do not appear to have that milkiness which generally attaches to nebulæ, and the bright nebulous lines are seen in some cases absolutely without any trace of continuous spectrum. In higher stages the continuous spectrum comes in, and in higher stages still possibly also the bands of carbon; for in many cases Dr. Huggins in his important observations has recorded the weakness

of the spectrum in the red, or in other words the strengthening of the spectrum in the green and blue, exactly where the carbon bands lie.

But in all the bodies of Group I which possess forms visible to us in the telescope, it would seem proper that their classification should depend mainly—at present at all events—upon their telescopic appearance, and there is very little doubt that a few years' labour with the new point of view in the mind of observers armed with sufficient optical power, will enable us to make a tremendous stride in this direction; but it seems already that this must not be done without spectroscopic aid. For instance, if what I have previously suggested as to the possible origin of the planetary nebulæ be accepted, it is clear that in those which give us the purest spectrum of lines—one in which there is the minimum of continuous spectrum—we find the starting point of the combined telescopic and spectroscopic classification, and the line to be followed will be that in which, *cœteris paribus*, we get proofs of more and more condensation and, therefore, more and more collisions, and therefore higher and higher temperatures, and therefore greater complexity in the spectrum until at length "stars" are reached.

When true stars are reached those in a cluster may appear nebulous in the telescope in consequence of its distance; the spectroscope must give us indications of absorption.

It is not necessary in this connexion, therefore, to refer to undoubted star clusters, as the presence of absorption will place them in another group; but the remark may be made that it is not likely that future research will indicate that new groupings of stars, such as Sir Wm. Herschel suggests in his paper on the breaking up of the Milky Way, will differ in any essential particular from the successive groupings of meteorites which are watched in the nebulæ. Space and gravitation being as they are, it is not necessary to assume that any difference of kind need exist in the groupings formed by stars and meteoric dust; indeed there is much evidence to the contrary.

### II. Sub-Group. Bright-line Stars.

It might appear at first sight that the distribution of bright-line stars among various species should be very easy, since a constant rise of temperature should bring out more and more lines, so that species might be based upon complexity of spectrum merely.

But this is not so, for the reason that the few observations already recorded, although they point to the existence of carbon bands, do not enable us to say exactly how far the masking process is valid. Hence in the present communication I content myself by giving some details relating to masking, and the results of the discussions, so far as they

have gone, in the case of each star. I shall return to the line of evolution of these bodies in a later paper.

*Masking of Radiation Effects produced by Variations of Interspacing.*

I have already stated that carbon bands are apt to mask the appearance of other spectral phenomena in the region of the spectrum in which they lie. In this way we can not only account for the apparent absence of the first manganese fluting, while the second one is visible, but it is even possible to use this method to determine which bands of carbon are actually present. There is another kind of masking effect produced in a different way, and this shows itself in connexion with sodium. It is well known that when the temperature is low, D is seen alone, and if seen in connexion with continuous spectrum the continuous spectrum is crossed by either dark or bright D, according to the existing circumstances.

I showed some years ago that the green line of sodium (but not the red one) is really visible when sodium is burned in the bunsen burner. It is, however, very much brighter when higher temperatures are used although when bright it does not absorb in the way the line D does.

Now, if we imagine a swarm of meteorites such that in the line o sight the areas of meteorite and interspace are equal, half the are will show D absorbed, and the other half D bright; and in th resulting spectrum D will have disappeared, on account of th equality, or nearly equality, of the radiation added to the absorptio of the continuous spectrum. The light from the interspace just fill up and obliterates the absorption.

But if the temperature is such that the green line is seen as well a D; in consequence of its poor absorbing effect there will be no dark lin corresponding to it in the resulting spectrum, but the bright gree line from the interspace will be superposed on the continuous spec trum, and we shall get the apparently paradoxical result of the gree line of sodium visible while D is absent. This condition can be partl reproduced in the laboratory by volatilising a small piece of sodiun between the poles of an electric lamp. The green line will be see bright, while D is dark.

In the bodies in which these phenomena apparently occur—for s far I have found no other origin for the lines recorded as 569, 570, an 571, the wave-length of the green sodium line being 5687—such a Wolf and Rayet's three stars in Cygnus and in $\gamma$ Argûs, the continu ous variability of D is one of the facts most clearly brought out b the observations, and it is obvious that this should follow if from an cause any variation takes place in the distance between the mete orites.

*In all meteoric glows* which have been observed in the laborator *not only D but the* green line has been seen constantly brig

while we know that in Comet Wells most of the luminosity at a certain stage of the comet's history was produced by sodium. It is therefore extremely probable that the view above put forward must be taken as an explanation of the absence of D when not seen, rather than an abnormal chemical constitution of the meteorites—that is to say, one in which sodium is absent. This may even explain the fact that up to the present time the D line of sodium has not been recorded in the spectrum of any nebula.

[*Note.*—In the lecture the author here referred to the spectrum of *o* Ceti, as photographed by Professor Pickering for the Henry Draper Memorial, the slide having been kindly placed at his disposal by the Council of the Royal Astronomical Society. All the bright hydrogen lines in the violet and ultra-violet are shown in the photograph, with the exception of the one which is nearly coincident with H. The apparent absence of this line is in all probability due to the masking effect of the absorption line of calcium. In this case, then, it appears that the calcium vapour was outside the hot hydrogen, and this therefore was being given off by the meteorites at the time.—April 18.]

*Detailed Discussions of the Spectra of some Bright-line Stars.*

These things then being premised, I now submit some maps to the Society illustrating this part of the inquiry, although it will be some time before my investigations on the bright-line stars are finished. These maps will indicate the way in which the problem is being attacked, and the results already obtained. To help us in the work we have first of all those lines of substances known to exist in meteorites *which are visible at the lowest temperatures which we can command in the laboratory.* We have also the results of the carbon work to which reference was made in the previous paper; and then we have the lines which have been seen, although their wave-lengths have in no case been absolutely determined in consequence of the extreme difficulty of the observation, both in stars and in comets, which I hold to be almost identical in structure.

In the case of each star the lines which have been recorded in its spectrum are plotted in the way indicated in the maps. The general result is that when we take into account the low temperature radiation, which we learn from the laboratory work, not only can we account for the existence of the lines which have been observed, but apparent absorptions in most cases are shown to be coincident with the part of the spectrum in front of a bright fluting.

A continuation of this line of thought shows us also that, when in these stars the spectrum is seen far into the blue, the luminosity really proceeds first *from the carbon* fluting, and in the hotter stars, *from the hydrocarbon one, which is still* more refrangible, in addition. *In the stars which have been examined* so far, the dark parts of the



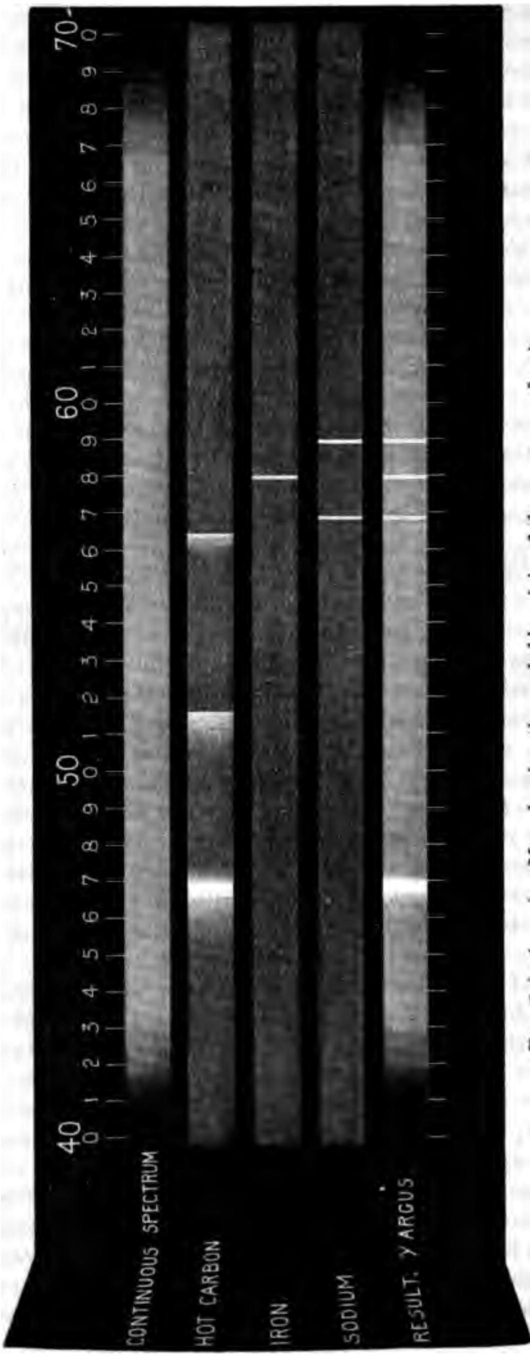



FIG. 5 (γ Argus).—Map showing the probable origin of the spectrum of γ Argus.

spectrum, which at first sight appear due to absorption, are shown to be most likely caused by defect of radiation in that part of the spectrum between the blue end of the continuous spectrum from the meteorites, and the bright band of carbon.

All the observations, it would appear, can be explained on the assumption of low temperature.

*Notes on the Maps.*

*γ Argus.*—R.A. 8 h. 5 m. 56 s., Dec. −46° 59·5′. Respighi and myself observed the bright lines in the spectrum of this star at Madras in 1871. No measurements were made of the wave-lengths of the lines, which were observed by Ellery at Melbourne in 1879, and given as 5760, 5648, and 4682. Other bright lines were suspected.

Copeland examined and mapped the spectrum of this star while in the Andes in 1883. His wave-lengths are 580·9, 566·8, 464·6, and a fainter line at 590. The continuous spectrum extends from 420 to 675, the lines being seen bright on this, but no mention is made by either Ellery or Copeland of absorption of any kind. The bright lines at 590 and 566·8 are most probably the lines of sodium, 5890—95 and 5687; the 580·9 line is probably the 579 strongest low-temperature line of iron; and the 468 (464·6 Copeland) is due to the carbon fluting, which has its maximum intensity at 468, the other carbon flutings at 517 and 564, being rendered invisible to Copeland by the bright continuous spectrum, although Ellery's measurement of 564·8 is most probably the carbon band at that point. The 517 carbon may have been seen by Ellery, for although no measurements are given he saw other bright lines or spaces. The dark band 474 to 486 seen in the Cygnus stars, Argelander-Oeltzen 17681, and Lalande 13412, being due to the shortness of the continuous spectrum, and the appearance of the carbon band beyond the blue end, is not seen in this star, because it has a long continuous spectrum.

The bright lines seen in it are due to low temperature sodium and iron, and to carbon flutings on a bright continuous spectrum.

Respighi's observations are given in 'Comptes Rendus,' vol. 74, p. 516; Ellery's results are given in a letter to 'The Observatory' vol. 2, p. 418; Copeland's are published in 'Copernicus,' vol. 3, p. 204.

*Argelander-Oeltzen* 17681.—Two observers have examined and mapped the spectrum of this star, Dr. Vogel at Potsdam, and Professor Pickering at Harvard College. Both give the wave-lengths of the lines observed, while in addition Dr. Vogel publishes a sketch of the spectrum as it appeared to him.

Vogel's strongest line is at 581. This Pickering measures as



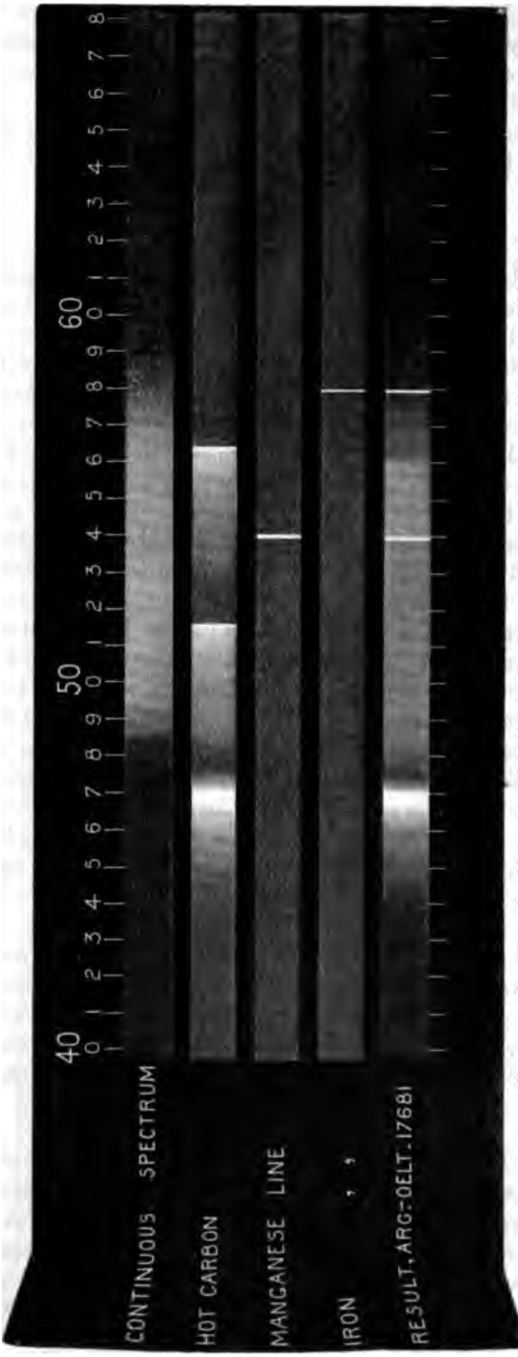



Fig. 6 — Map showing the probable origin of the spectrum of Argelander-Oeltzen 17681.

590–585, evidently when using a wide slit, while in a later account of his observations he fixes the wave-length at 580. The line is probably 579, the strongest line of iron at a low temperature. Vogel mentions a bright band extending from 470 to 461 with a maximum between these limits. Pickering measures this as commencing at 473. This band is evidently the bright band of carbon commencing at 474, with a maximum about 468 as observed and photographed at Kensington. Between this band and 486 Vogel has shown a dark band in the spectrum. This appearance is due not to any absorption but to the continuous spectrum being short, ending evidently at 486, while the bright carbon appearing beyond this in the blue, leaves a dark band due to absence of radiation.

Vogel has not noticed any other bright lines, but Pickering "suspected" a brightening at 540. This would be the only line of manganese which appears in the bunsen burner. Vogel may have noticed this line and yet not given any wave-length of it in his list, just as he indicates one bright line in 2nd Cygnus, and two bright lines in 3rd Cygnus in his light curves of those stars, without mentioning them in any list of bright lines observed.

Pickering suspected the presence of several other lines, but was unable to obtain any measurements of them.

Vogel's results are given in the 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 15, and in the sketch at the end of that number.

Pickering's are in 'The Observatory,' vol. 4, p. 82; the 'American Journal of Science and Art,' No. 118, 1880; 'Copernicus,' vol. 1, p. 86; and 'Astronomische Nachrichten,' 2376.

*Lalande* 13412—Both Vogel and Pickering have observed the spectrum of this star and have measured the wave-lengths of the bright lines.

Vogel gives a sketch of the spectrum as well as a list of wave-lengths.

Vogel mentions a dark band at the blue end of the spectrum, and gives the wave-length in his sketch as from 486 to 473.

Both observers measure the bright 486 hydrogen (F) line.

Vogel measures a bright line at 540, while Pickering's measure is 545; but Pickering in another star, Arg.-Oeltzen 17681, has measured a line at 540, so there can be little doubt that is the correct wave-length.

Vogel measures a line at 581, but this has not been noticed by Pickering.

The bright *part of the spectrum* extending from 473 towards the *blue with its maximum at 468 is,* I would again suggest, the carbon *band appearing beyond the continuous* spectrum, the rest of the carbon

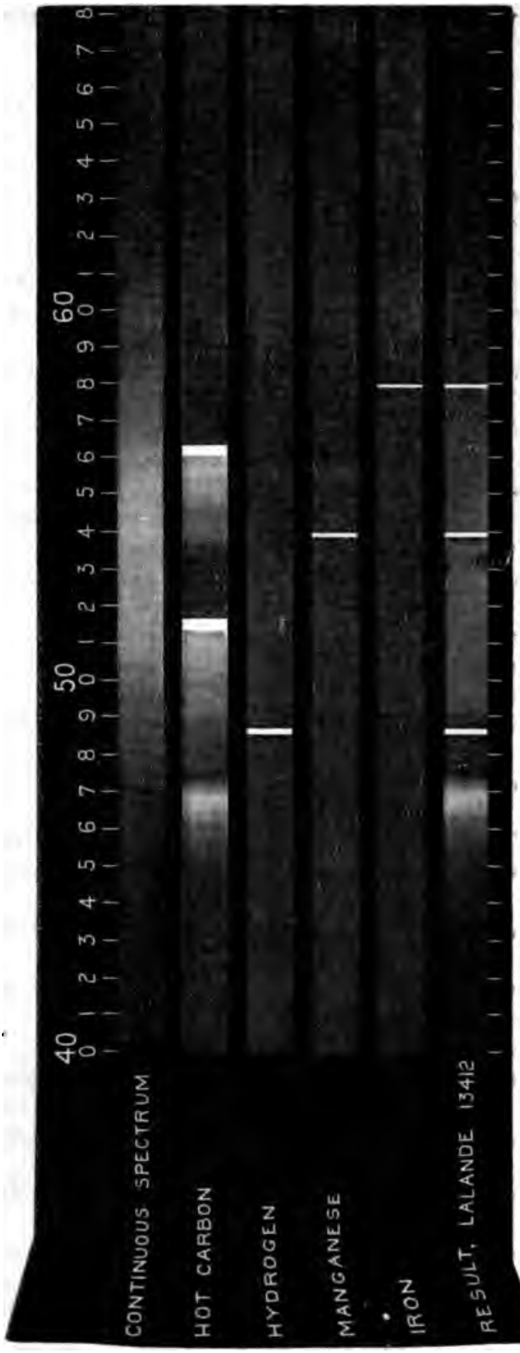



Fig. 7.—Map showing the probable origin of the spectrum of Lalande 13412.

being cut out by the continuous spectrum, although 564 asserts itself by a brightening of the spectrum at that wave-length in Vogel's sketch, and by a rise in his light curve.

The line at 540 is the only line of manganese visible at the temperature of the bunsen burner, while the 581 measurement of Vogel is in all probability the 579 line, the strongest line of iron visible at low temperatures.

In this star, therefore, we have continuous spectrum from the meteorites, and carbon bands, one of them appearing beyond the continuous spectrum in the blue as a bright band; bright lines of hydrogen, manganese, and iron being superposed on both. There is no absorption of any kind, the apparent dark band being due to defect of radiation, as in Argelander-Oeltzen 17681.

Vogel's results are given in the 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 17.

Pickering's are published in the 'Astronomische Nachrichten,' No. 2376; 'Science,' No. 41; and quoted in 'Copernicus,' vol. 1, p. 140.

1*st Cygnus*.—B.D. + 35°, No. 4001.—The spectrum of this star was observed by Messrs. Wolf and Rayet in 1867, but no measurements of the positions of the bright lines were then published. In the same paper, however, they give the measurements of the positions of the bright lines in 2nd Cygnus (B.D. + 35°, No. 4013) which they observed about the same time, and since the bright lines were similar in these stars, the wave-lengths 581, 573, 540, and 470, may be taken as indicating the positions of the lines in 1st Cygnus. They also observed dark spaces between 470 and 486, and on the blue side of 573.

Dr. Vogel, of Potsdam, examined the spectrum of this star, and has published his results in three ways, as a list of bright lines given in wave-lengths; as a sketch of the spectrum as it appeared to him, and as a curve showing the intensity of the light throughout the spectrum.

His wave-lengths are 583, 571, 541, 486 (hydrogen F) for lines, and a bright band from 470 to 465, with its maximum at 468.

The sketch confirms these lines, while the light curve adds three others to them at wave-lengths 507, 527, and 558. He also gives an absorption-band between the 486 line and 470 band, and in his sketch gives a darkening on the blue side of 570, this being also indicated in the light curve. These dark spaces agree with the dark spaces observed by Messrs. Wolf and Rayet.

The bright band, with its maximum at 468, is the bright carbon fluting commencing at 474, and extending towards the blue with its maximum at 468, as photographed at Kensington, and the dark space

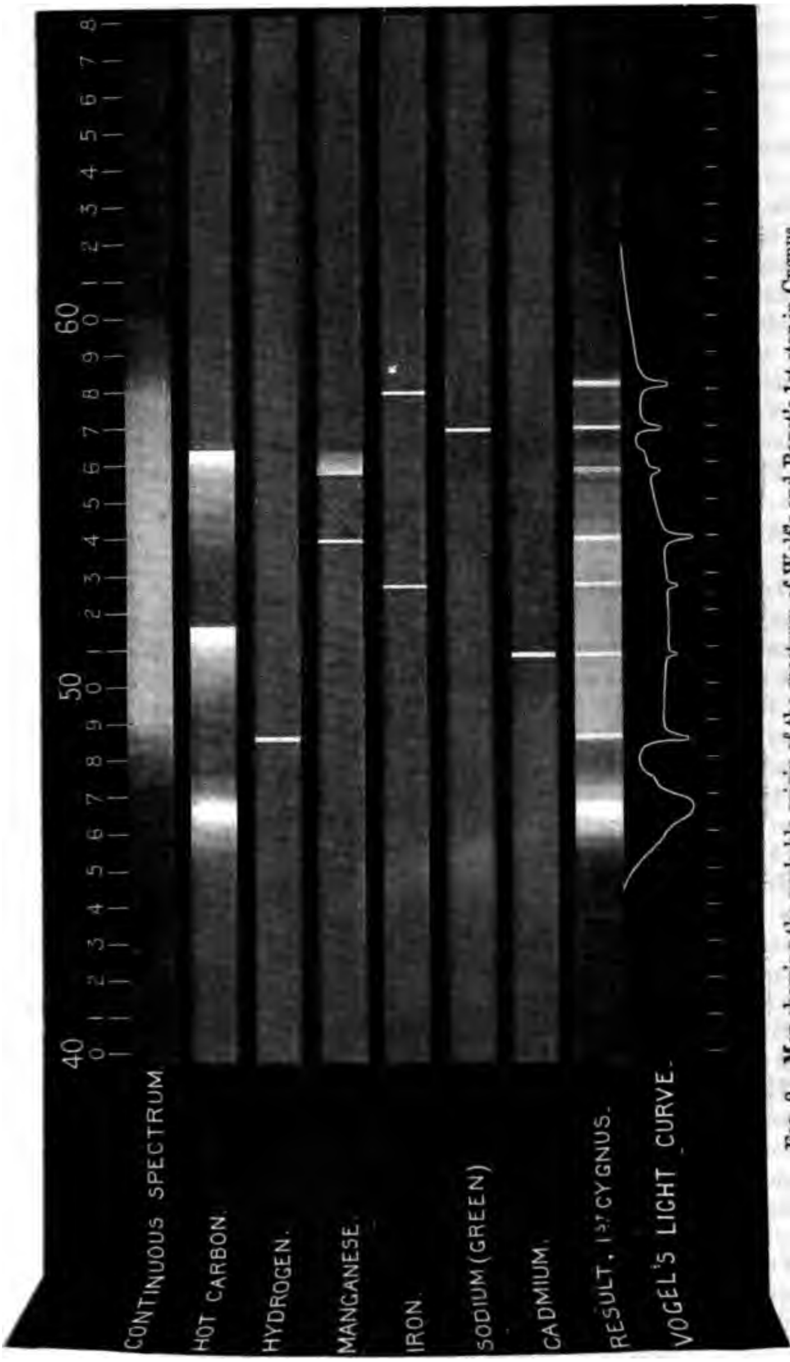



FIG. 8.—Map showing the probable origin of the spectrum of Wolf's and Rayet's 1st star in Cygnus.

ıetween this and the 486 line is not due to absorption of the light rom the meteorites by any vapour around them, but rather to the bsence of any radiation except that from the meteorites themselves t this part of the spectrum.

The carbon at 564 raises the curve at that point, and this brightess with the bright 570 line produces the appearance of a dark pace between those wave-lengths, the band being simply due to the ontrast of a bright fluting and a bright line lying some distance part on a faint continuous spectrum. There is therefore no bsorption of any kind in this star, all the dark bands being due to bsence of radiation.

Of the bright lines two, the 540 and the 558, are due to manganese, 40 being the manganese line visible in the bunsen, while 558 is the trongest of the low temperature flutings of manganese. The line t 581, or thereabouts, is most probably the strongest low temperature .ne of iron. The line at 569 is most probably the green sodium .ne, while the 486 line is assigned by Vogel to hydrogen. The faint .ne at 507 has been observed in the flame spectra of several meteor:es, and is in the exact position of the strongest line of cadmium t the temperature of the bunsen burner.

This star, therefore, gives a spectrum, which is short and faintly ontinuous, due to radiation of meteorites, but has light from carbon dded, with a separate band appearing in the blue; while the strongest ɔw-temperature lines of manganese, iron, and cadmium, with a trong manganese fluting, and the green sodium line, appear bright n the continuous spectrum. There is no absorption of any .ind.

Wolf and Rayet's discovery of bright lines is recorded in 'Comptes Rendus,' vol. 65, p. 292, and confirmed in vol. 68, p. 1470, vol. 69, p. 39 and 163. Vogel's observations are given in the 'Publicationen es Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, . 17, and shown in a sketch at the end of that number.

*2nd Cygnus.*—B.D. + 35°, No. 4013.—Messrs. Wolf and Rayet, in 867, first observed the spectrum of this star, and measured the ositions of the bright lines. Micrometer readings and reference nes are given by them from which a wave-length curve has been onstructed. The wave-lengths of the bright lines in the star thus scertained are: 581 ($\gamma$), 573 ($\beta$), 540 ($\delta$), and 470 ($\alpha$); the elative intensities being shown by the Greek letters. They state hat:—

"La ligne $\beta$ est suivie d'un espace obscur; un autre espace trèsombre précède $\alpha$."

*Vogel afterwards examined the* spectrum, measured the positions ıd *ascertained the wave-lengths* of the bright lines, drew a sketch

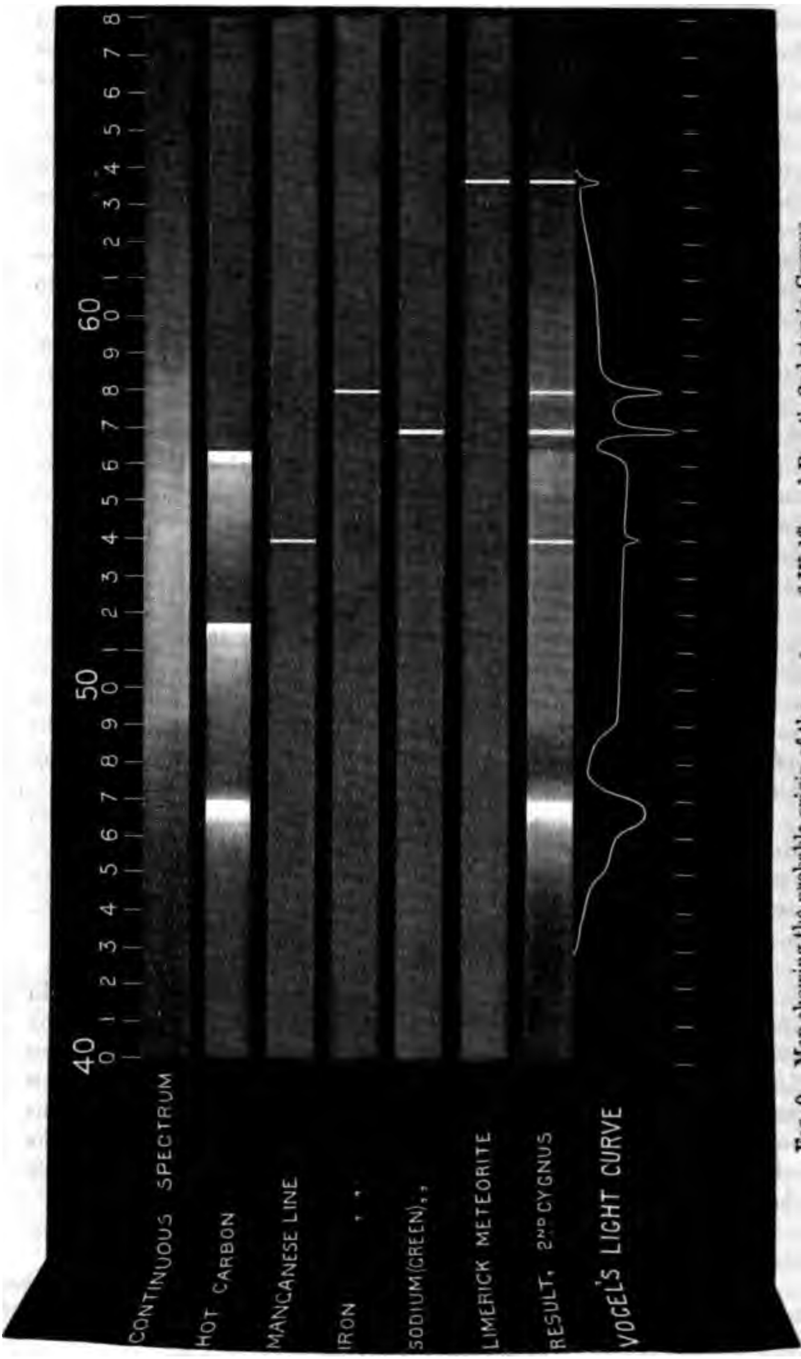



Fig. 9.—Map showing the probable origin of the spectrum of Wolf's and Rayet's 2nd star in Cygnus.

of the spectrum as it appeared to him, and a curve showing the variation of intensity of the light throughout the spectrum.

The wave-lengths given by Vogel are 582 and 570, and a band with its brightest part at 464, fading off in both directions and according to the sketch having its red limit at 473. In the light curve Vogel not only shows the 582 and 570 lines, but also bright lines in positions which by a curve have been found to correspond to wave-lengths 540 and 636. Vogel indicates in his sketch a dark band extending from 486 to the bright band 473, and an apparent absorption on the blue side of the 570 line, this absorption being ended at 564. These two bands agree in position with the dark spaces observed by Messrs. Wolf and Rayet. The bright band in the blue at 473 is most probably the carbon band appearing bright upon a faint continuous spectrum, this producing the apparent absorption from 486 to 473. If the bright carbon really accounts for the appearance of a (contrast) dark band between the bright 570 and 564 in this star, all the apparent absorption is explained as due to contrast of bright bands on a fainter continuous spectrum due to red-hot meteorites.

The line at 540 is the only line of manganese visible in the bunsen burner, and the 580 line is the strongest low-temperature iron line. The 570 line is most probably the green sodium line 569, the absence of the yellow sodium being explained by the half-and-half absorption and radiation mentioned in the discussion of the causes which mask and prevent the appearance of the lines in a spectrum.

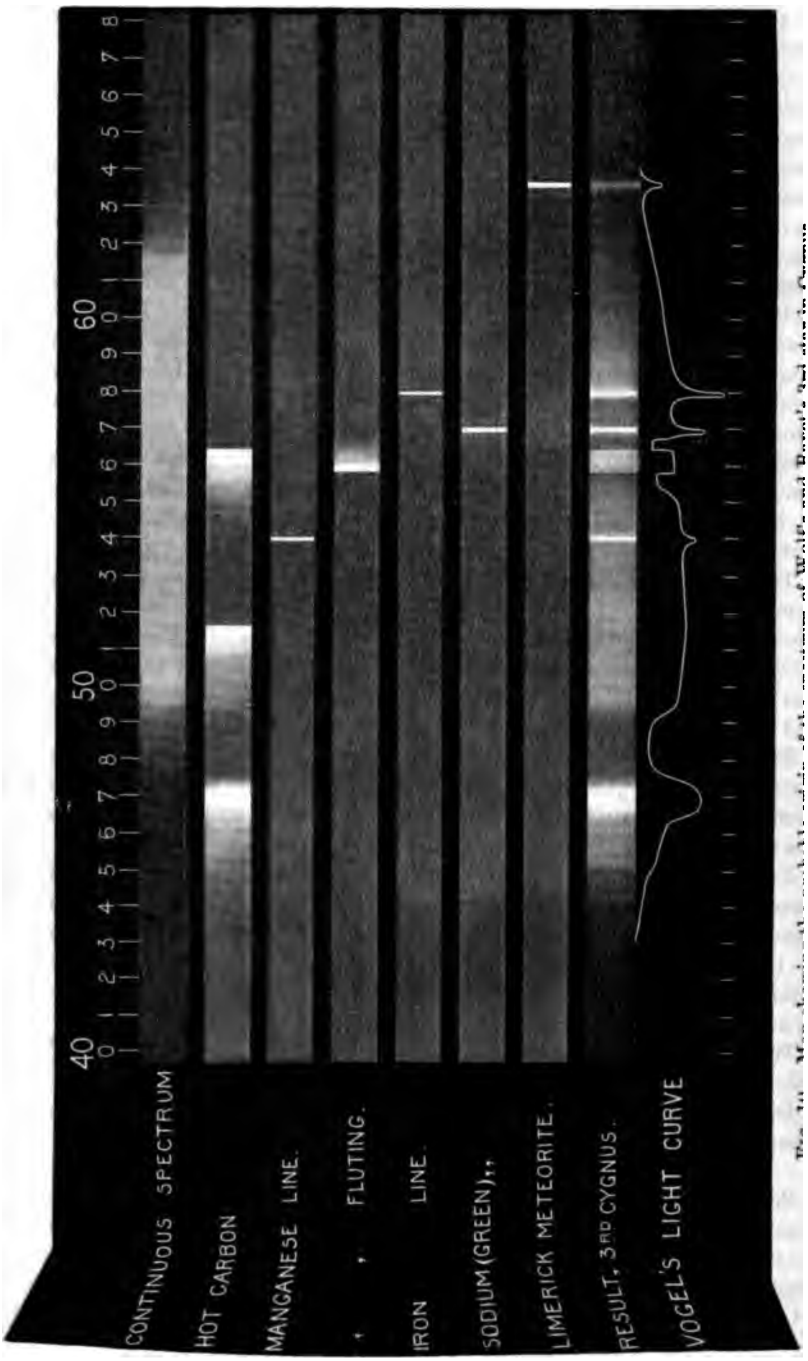
The line at 636 is in the red just at the end of the continuous spectrum, and as yet no origin has been found for it, although it has been observed as a bright line in the Limerick meteorite at the temperature of the oxyhydrogen blowpipe.

This star therefore gives a continuous spectrum due to radiation from meteorites, and on this we get bright carbon (with one carbon band appearing separate as being beyond the continuous spectrum in the blue), with bright lines of iron, manganese, sodium, and some as yet undetermined substance giving a line at 636 in the oxyhydrogen blowpipe.

Wolf and Rayet's results are given in the 'Comptes Rendus,' vol. 65, p. 292.

Dr. Vogel's are from the 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 19.

*3rd Cygnus.*—B.D. + 36°, No. 3956.—This is one of the three stars observed by Messrs. Wolf and Rayet, in 1867, as having bright lines in their spectra, but *they do not give measurements of the wave-lengths of the lines. They give*, however, lines at 581, 573, 540, and *470, as present in 2nd Cygnus*, so we can reasonably infer these wave-


CONTINUOUS SPECTRUM
HOT CARBON
MANGANESE LINE.
" FLUTING.
IRON LINE.
SODIUM (GREEN) ,,
LIMERICK METEORITE.
RESULT, 3RD CYGNUS.
VOGEL'S LIGHT CURVE
40
50
60

lengths are fairly correct for this star, especially as Dr. Vogel's measurements of the bright lines are 582 and 569 with a bright band commencing at 468. Vogel, in addition to his wave-lengths, also gives a sketch of the spectrum in which he shows the bright 540 line; and a light curve showing the variations of the intensity of the light throughout the spectrum, in which curve he indicates all the lines above-mentioned, and an additional bright line at 636.

The sketch shows also a dark band in the spectrum from about 488 to 473, another from 553 to 556, and a third on the blue side of 570 extending from that line to 564. These dark spaces are confirmed in the light curve, and two of them, 488 to 473, and 570 to 564, agree with the dark spaces observed by Messrs. Wolf and Rayet in 2nd Cygnus.

The bright band at 470 is the carbon band in the blue commencing at 474, with its maximum at about 468, as observed and photographed at Kensington, and between this and 488 is the dark space which is most probably due to absence of radiation rather than to any absorption. The carbon at 517 asserts itself by a rise in the light curve at that point, while the 564 carbon is also seen to produce a sudden rise in the curve.

The 564 carbon and the 558 manganese fluting uniting produce a bright band of light between those wave-lengths, and this on the faint continuous spectrum produces an apparent dark space on each side, thus accounting for the dark appearances at 554—557 and 564—570, these being contrast appearances only and not absorption bands. The 540 line is the manganese line seen in the bunsen burner. The line at 570 is most probably the green sodium line, the yellow sodium being rendered invisible by the half-and-half absorption and radiation masking previously mentioned. The 580 line is most probably the strongest low-temperature line of iron, 579; while the 636 line has been seen in the Limerick meteorite when heated in the oxyhydrogen flame, although its origin has not yet been determined.

In this star, therefore, we have continuous spectrum from the meteorites; carbon bands at 474, 517, and 564, rendering themselves apparent in the light curve; the low-temperature manganese line and the strongest manganese fluting; the low-temperature iron line, the green sodium, and a line the origin of which is unknown, all appearing bright. There is no absorption.

Vogel's results are given in the 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' vol. 4, No. 14, p. 19.

*γ Cassiopeiæ*.—Secchi at the very commencement of his work at stellar spectra noticed the bright lines in the spectrum of this star. He records the presence of bright lines of hydrogen and of the bright

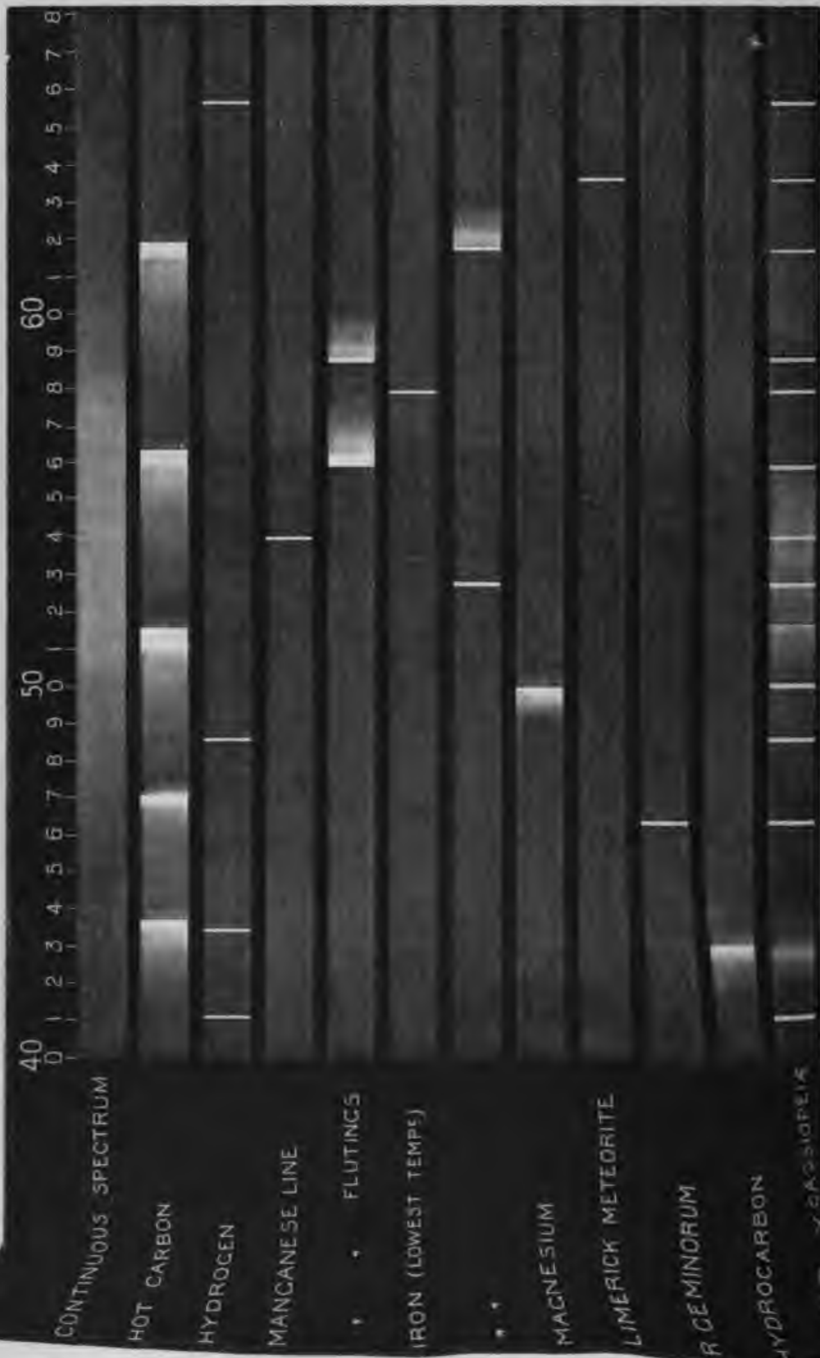



FIG. 11 (γ Cassiopeiæ).—Map showing the probable origin of the spectrum of γ Cassiopeiæ.

$D_3$ line. ('Bull. Météorol. du Collège Romain,' 31 Juillet, 1863, p. 108.)

Vogel on June 19th, 1872 observed a bright line in the greenish-blue 486, and one in the yellow which he assumes to be $D_3$. An absorption band was also noticed in the red, but its wave-length was not determined. ('Both. Beob.,' Heft 2, p. 29.)

Great stress was laid on the fact that the bright lines died out between 1874 and 1883, when they were observed by Gothard, but on December 26th, 1879, C was noted as "superbly visible" by Lord Lindsay, J. G. Löhse and Dr. R. Copeland, and two bright lines, one evidently F, observed on October 28th, 1877. No mention is made of C in the records of the observation. ('Monthly Notices of the R. Astron. Soc.,' vol. 47, p. 92.)

Konkoly examined $\gamma$ Cass. (and $\beta$ Lyræ) repeatedly between 1874 and 1883, without seeing bright lines; Gothard repeatedly examined both stars after the autumn of 1881, but saw no trace of bright lines until 1883. ('Astr. Nachr.,' 2581.)

The Greenwich observations for October 1st, and November 21st, 1880, December 7th, 1881, and November 16th, 1883, show the F line bright. No mention is made of bright $D_3$ or C, but only F was being used to measure velocity in line of sight, and so the others may not have been particularly noted.

Gothard, in 'Astr. Nachr.,' No. 2539, records his observations on August 20th, 1883, when C, F, $D_3$, and the absorption band at 633 were visible.

Konkoly took up this work at once, and in the O'Gyalla Observations we find two sketches of the spectrum as seen by him. In the first C and F are bright lines sharply defined. $D_3$ is seen as a bright line, while between $D_3$ and F is a bright patch of light extending from near 520 to 560. This seems to be absent in the second spectrum, while dark *b* lines and dark D are added as well as bright hydrogen G with a dark line near it.

Sherman at Yale College Observatory records all the bright lines previously observed and many others in addition, but while dark lines are recorded by him, D and *b* are not mentioned.

Gothard ('Astr. Nachr.,' No. 2881) has observed H$\alpha$, H$\beta$, and H$\gamma$ as dark lines in $\beta$ Lyræ, and afterwards as bright lines.

Sherman's observations, in which no mention is made of dark D lines, are of extreme interest, indicating as they do that the sodium line absorption was masked by the bright radiation of manganese, which produces a bright fluting almost exactly in the position of $D_3$. Gothard, in 'Astr. Nachr.,' No. 2581, records the fact that the dark sodium lines *became visible* only when $D_3$ had ceased *to be seen as a bright line. Later* on in the same paper, however, *he records bright $D_3$ and dark D* in $\beta$ Lyræ, and Konkoly, in

Table of Bright Lines in $\gamma$ Cassiopeiæ.

| Secchi. | Vogel. | Huggins. | Gothard. | Konkoly. | Sherman. | Probable orig |
|---|---|---|---|---|---|---|
| C. | .. | C. | C. | C. | C. | H. |
| | | | | | 635·6 | (?) Limerick |
| | | | | | 616 | Fe. |
| $D_3$. | $D_3$. | $D_3$. | $D_3$. | $D_3$.* | $D_3$. } | Mn. |
| | | | | | 584 ? } | |
| | | | | | 555·75 | Mn. |
| | | | | | 542·2 | Mn. |
| | | | | | 530·98 | (?) Coronal li |
| | | | | | 516·75 | C. |
| | | | | | 499 | Mg. |
| F. | F. | F. | F. | F. | F. | H. |
| | | | | | 462·3 | |
| | | | | G. | G. | H. |
| | | | | | 418 | |
| | | | | | *h*. | H. |
| | | | | Dark Lines. | | |
| | | | | 666·2—656 | | |
| | | | 633 | 659·0—624 | 628 | |
| | | | 589 | 589 | | D. |
| | | | | | 576 | |
| | | | 517 (*b*) | 516 (*b*) | | *b*. |
| | | | | | 502 | |
| | | | | | 492 | |
| | | | | 431 | 467·35 | |
| | | | | | 399·3 | |

vol. 6 of the O'Gyalla Observations, records the same in $\gamma$ Cassiop When we consider the great variations in brightness of $D_3$ in tl stars and the great changes in the conditions of the radiat meteorites and their atmospheres, indicated by these changes brightness, these apparently discordant results are not so difficul understand. An increase in the number of meteorites containing would cut out all the D absorption and brighten $D_3$; an increas sodium and a decrease of Mn would cause the D dark lines to as themselves, while the condition of bright $D_3$ and dark D is obtai by increased quantities of Mn and Na vapours produced by collisio

Sherman does not record dark *b* lines, although Konkoly obser them several times. Sherman, however, saw the bright carbon 517, which would completely mask the *b* lines. It seems poss Konkoly saw this bright carbon, and by contrast with the surround spectrum, imagined he saw the dark "*b*" lines—at any rate no ot observer has recorded dark *b*.

*Sherman saw the* magnesium 500, while neither Konkoly

* *Konkoly's* $D_3$ *extends* quite up to D dark and seems more like a fluting t *right line.*

Gothard noticed it; so after all it may be probable that Konkoly's record of magnesium absorption at *b* was right, and that in Sherman's observation it was masked by the carbon band.

Sherman, in 'Astr. Nachr.,' No. 1707, gives a list of fifteen bright lines in $\gamma$ Cassiopeiæ, the wave-lengths of which he has determined as accurately as possible. He says, "the difficulties of the observation and the roughness of the recording apparatus have hindered the completely satisfactory identification of the lines. Assuming the position of the hydrogen lines and $D_3$, and on their basis constructing a curve connecting scale-reading and wave-length, the mean of nine observations upon $\gamma$ Cassiopeiæ affords the following approximate wave-lengths." (See map.)

The line in the yellow being assumed as $D_3$ at 5875, instead of the 5870 manganese, causes an error running all through the measurements, but not sufficient to invalidate any conclusions based on the corrected wave-lengths.

The hydrogen lines seen are C, F, hydrogen G, and *h*. We have the manganese at 558 and 586 ($D_3$), as well as the low-temperature line (bunsen) at 540. Iron is represented by lines at 527, 579, and 616, these being the strongest low-temperature lines. Magnesium is responsible for the 500 line while the carbon accounts for the 517, thus leaving only the 636 and the 463 lines unaccounted for.

The line at 636 has been seen in the Limerick meteorite, although its origin has not yet been determined, while the 463 line is bright in R Geminorum, but has up to the present not been detected in any experiment with meteorites. In the spectrum of the first of Wolf and Rayet's stars in Cygnus (B.D. 35°, No. 4001), Vogel has observed the manganese lines at 540 and 558, the iron lines at 527 and 579, and the hydrogen F, all of which are present in $\gamma$ Cassiopeiæ, the only additional lines seen in 1st Cygnus being the sodium green, 569, and cadmium, 507.

*On the Sequence of Temperature of the Stars in Cygnus.*

The three "bright line stars" in Cygnus, discovered by MM. Wolf and Rayet in 1867, present differences in their spectra, which raise some very interesting questions for discussion. Wolf and Rayet did not observe any great differences in the spectra, simply recording the fact that the second star gave the lines most brilliantly; but Dr. Vogel has, in his investigations, brought out very striking ones.

Thus the first of these stars, B.D. + 35°, No. 4001, has seven bright lines in its spectrum, as shown on his light curve, besides the bright band at 468. One of the bright lines is hydrogen F (486). The second, B.D. + 35°, No. 4013, and third, B.D. + 36°, No. 3956, stars have only four bright lines, and the bright band; the hydrogen (F) line being absent.

These differences may at first sight be taken as indicating a higher temperature in the first of these stars than in either of the others, but further investigation seems to indicate this is not the case. The continuous spectrum from the meteorites is very faint in each case, and on it is superposed bright carbon, that in the blue showing itself as a separate bright band, 468. The curve rises in each star at 564 carbon, and is high in the position 517.

It will be seen from the light curves that the rise at 564 is less in 1st Cygnus than in either of the other stars, and the end of the fluting 558, due to the manganese, becomes visible as a line in this star, while in 2nd and 3rd Cygnus the carbon at 564 with this fluting produces such a brightening of the spectrum that the manganese cannot be seen as a bright line. In 2nd Cygnus the 564 carbon is nearly equal in brightness to the 558 manganese fluting, and these produce together such an intensely bright patch between those wave-lengths that we get apparent dark spaces on each side of it. The 540 line of manganese has a considerable difficulty in showing itself on the bright spectrum due to meteorites and carbon combined, whereas in 1st Cygnus where the radiation of carbon is weaker the line is very bright. The invisibility of 507 and 527 in the spectra of 2nd and 3rd Cygnus stars is therefore due probably to the extra brightness of the fluting spectrum due to carbon, rather than to the lower temperature of these stars. The greater number of lines in 1st Cygnus indicates therefore a lower temperature than in the other stars, and this conclusion is borne out by the appearance of the 636 line in 2nd and 3rd Cygnus, and its absence from 1st Cygnus.

The conclusion which has been arrived at after a careful consideration of these stars is that 1st Cygnus is the coolest, 2nd Cygnus ranks next above in temperature, and 3rd Cygnus is the hottest of the three.

With regard to the line in 2nd and 3rd Cygnus at 636 there is an element of doubt as to the true position. Vogel does not give the wave-length in his list of lines, neither does he show it in his sketch of the spectrum, but he indicates its position on the light curve, and from this a curve had to be drawn and the wave-length ascertained as nearly as possible. Vogel suggests the line may be the hydrogen C line, but this seems very improbable, since F is absent, and although F is frequently recorded in bright-line stars without C, in no case is C given without F. It may be the C line is seen clearly because there is no continuous spectrum near it, while F is not visible on account of the bright spectrum around it.

The above stars are not the only ones with bright lines in the constellation Cygnus.

*A recent* communication by Professor Pickering gives the following *additional information* :—*

* 'Nature,' September 9, 1886.

A recent photograph of the region in Cygnus, previously known to contain four spectra exhibiting bright lines, has served to bring to our knowledge four other spectra of the same kind. One of these is that of the comparatively bright star P Cygni, in which bright lines, apparently due to hydrogen, are distinctly visible. This phenomenon recalls the circumstances of the outburst of light in the star T Coronæ, especially when the former history of P Cygni is considered. According to Schönfeld, it first attracted attention, as an apparently new star, in 1600, and fluctuated greatly during the seventeenth century, finally becoming a star of the fifth magnitude, and so continuing to the present time. It has recently been repeatedly observed at the Harvard College Observatory with the meridian photometer, and does not appear to be undergoing any variation at present.

Another of the stars shown by the photograph to have bright lines is D.M. + 37° 3821, where the lines are unmistakably evident, and can readily be seen by direct observation with the prism. The star has been overlooked, however, in several previous examinations of the region, which illustrates the value of photography in the detection of objects of this kind.

The other two stars first shown by the photograph to have spectra containing bright lines are relatively inconspicuous. The following list contains the designations according to the 'Durchmusterung,' of all eight stars, the first four being those previously known:—35° 4001, 35° 4013, 36° 3956, 36° 3987, 37° 3821, 38° 4010, 37° 3871, 35° 3952 or 3953. Of these 37° 3171 is P Cygni, and 37° 3821, as above stated, is the star in the spectrum of which the bright lines are most distinct.

[Received March 28, 1888.]

## PART IV.—SUB-GROUPS AND SPECIES OF GROUP II.

### 1. GENERAL DISCUSSION OF DUNÉR'S OBSERVATIONS.

In the paper communicated to the Royal Society last November I pointed out that the so-called "stars" of Class III*a* were not masses of vapour like our sun, but swarms of meteorites; the spectrum being a compound one, due to the radiation of vapour in the interspaces and to the aborption of the light of the red- or white-hot meteorites by vapours volatilised out of them by the heat produced by collisions.

I also showed that the radiation was that of carbon vapour, and that some of the absorption was produced by the chief flutings of Mn and Pb.

These *conclusions were arrived at* by comparing the wave-lengths *of the details of spectra recorded* in my former paper with those of

the bands given by Dunér in his admirable observations on t bodies.*

Dunér in his map gives eleven absorption bands, chiefly fluti in Class III*a*, but in the case of the tenth and eleventh bands the some discrepancy between his map and the text, to which refer will be made subsequently. His measurements are of the da portions of the flutings, speaking generally.

It will be clear at once that in the case of the *dark* flutings the bands should agree with the true *absorption* of the vapours, and when the amount of absorption varies, only that wave-length a from the maximum of the flutings will vary. Thus, the same flu may be represented in the following manner, according to the quai of the absorbing substance present.

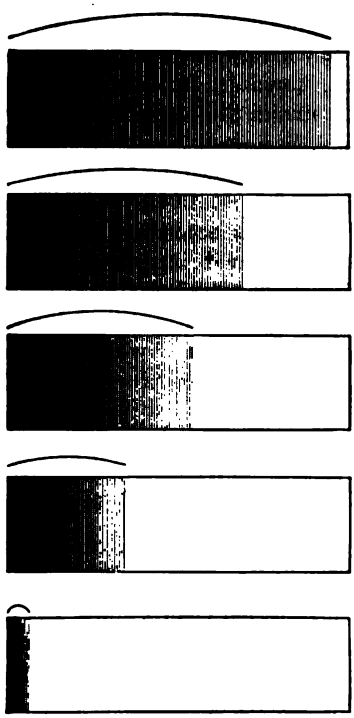


FIG. 12.—Diagram showing how an absorption fluting varies in width accordi the quantity of absorbing substance present.

In the case of the *bright* flutings, however, the dark bands on ei *side may in some cases* be produced partly by contrast only, and

* "*Les Étoiles à Spectres* de la troisième classe."—'Kongl. Svenska Veten *Akademiens Handlingar,*' Band 21, No. 2, 1885.

brighter and wider the bright flutings are the more the dark flutings on either side of them will appear to vary, and in two ways: first, they will dim by contrast when the bright fluting is dimmer than ordinary; and secondly, the one on the side towards which the bright fluting expands from its most decided edge will diminish as the bright fluting expands. See following diagram.

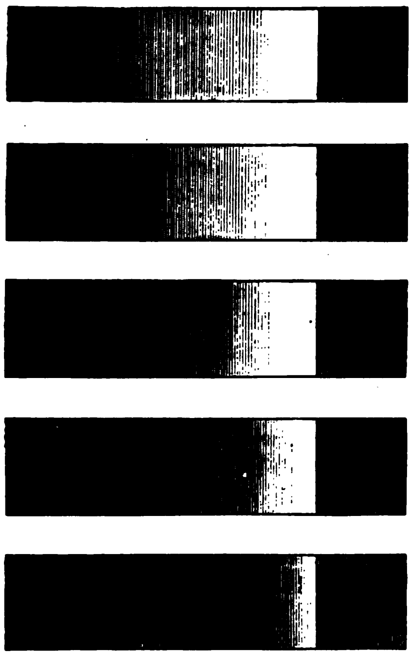


FIG. 13.—Diagram showing the variation in width of a bright fluting and the consequent variation in width of the contrast band at the fainter edge.

There is also another important matter to be borne in mind. As these spectra are in the main produced by the integration of the continuous spectra of the meteorites, the bright flutings of carbon, and the dark flutings produced by the absorption of the continuous spectra by the vapour surrounding each meteorite; the proportion of bright fluting area to dark fluting area will vary with the reduction of the spacing between the meteorites.

If any bright or dark flutings occur in the same region of the spectrum, when the spaces are greatest, the radiation effect will be stronger, and the absorption *fluting* will be "masked;" where they are *least the radiation itself will be* masked. This reasoning not only *applies to flutings but to lines also.*

### *The Radiation Flutings.*

We will first deal with the radiation flutings—those of carbon. The brightest less refrangible edge of the chief one is at wave-length 517, where it sharply cuts off the tail end of the absorption of the magnesium fluting the darkest edge of which begins at 520, as the carbon light from the interspace pales the absorption. The same thing happens at the more refrangible edge of the other absorption of Mg at 500, as Dunér's figures show.

| | Less refrangible edge. | | More refrangible sharp edge. |
|---|---|---|---|
| Band 8 (absorption of Mg) ........ | 502 | ...... | 496 in $\alpha$ Herculis. |
| | 501 | ...... | 496 in $\rho$ Persei. |
| | 503 | ...... | 496 in R Leonis Min. |
| | 505 | ...... | 496 in $\beta$ Pegasi. |

If this explanation of the rigidity of the less refrangible edge may be accepted, it is suggested that the rigidity of the end of band 8 at 496, near the nebula line 495, seems to indicate that we may have that line as the bright, less refrangible, boundary of another radiation fluting.

The fluting at 517 is the chief radiation fluting of carbon. The next more refrangible one, which would be most easily seen, as the continuous spectrum would be less bright in the blue, has its less refrangible and brightest edge at 474.

This in all probability has been seen by Dunér, though, as before stated, there is here a discrepancy between his maps and his text. It lies between his dark bands 9 and 10, the measurements of which are as follow:—

| | Less refrangible edge. | | More refrangible edge. |
|---|---|---|---|
| Band 9 .......... | 482 | ...... | 476 in $\alpha$ Orionis. |
| | 484 | ...... | 477 in $\beta$ Pegasi. |
| Band 10 ......... | 472 | ...... | 460 in $\alpha$ Orionis. |
| | 474 | ...... | 462 in $\alpha$ Herculis. |

It is not necessary for me to point out the extreme and special difficulty of observations and determinations of wave-lengths in this part of the spectrum. Taking this into consideration, and bearing in mind that my observations of the chemical elements have shown me no other bands or flutings in this region, I feel justified in looking upon the narrow bright space between bands 9 and 10 as an indication of another carbon fluting—the one we should expect to find associated with the one at 517, with its bright edge at 473 instead of 476, where Dunér's measurements place it. There is a bright fluting in this position in Nova Orionis.

I shall refer to both these points later on.

The third fluting, the carbon one with its brightest edge at 564, is certainly also present; though here the proof depends upon its masking effect, and upon the manner in which this effect ceases when the other flutings narrow and become faint.

In addition to these three flutings of carbon, which we shall distinguish in what follows as carbon A, there is sometimes a fourth more refrangible one beginning at wave-length 461, which is due to some other molecular form of carbon; this we shall distinguish as carbon B. It extends from wave-length 461 to 451, and, as we shall presently see, it is this which gives rise to the apparent absorption band No. 10 in the blue.

It is very probable also that in some cases there is, in addition to carbon A and carbon B, the hydrocarbon fluting which begins at wave-length 431, the evidence of this being Dunér's apparent absorption band 11. It may be remarked here, that although most of the luminosity of this fluting is on the more refrangible side of 431, there is also a considerable amount on the less refrangible side.

With regard to bands 9, 10, and 11, then, there is little doubt that they are merely dark spaces between the bright blue flutings of carbon, and that whether they are seen or not depends upon the relative brightness of the carbon flutings and the continuous spectrum from the incandescent meteorites. When the continuous spectrum is faint, it will not extend far into the blue, and the resulting dark space between the bright carbon A fluting at 474 and the end of the continuous spectrum is the origin of the apparent absorption band 9. When the continuous spectrum gets very bright, band 9 should, and does, disappear. On reference to the maps of the spectra of the "stars" with bright lines, it will be seen that the broad apparent absorption band in the blue agrees exactly in position with band 9, and it undoubtedly has the same origin in both cases. This band may therefore be regarded as the connecting link between the bodies belonging to Group I and those belonging to the group under consideration.

Band 10 is the dark space between the bright carbon A fluting at 474 and the carbon B at 461, and can only exist so long as the carbon flutings are brighter than the continuous spectrum. Dunér's mean values for the band are 461—473, and on comparing these with the wave-lengths of the carbon flutings (see fig. 16) it will be seen that the coincidence is almost perfect.

There is a little uncertainty about band 11, which Dunér was only able to measure in one star, but it very probably has its origin in the dark space between the bright carbon B fluting and the hydrocarbon fluting at 431 (see fig. 16). This would give a band somewhat broader and more refrangible than that shown in Dunér's map; but,

as already pointed out, great accuracy in this part of the spectrum cannot be expected.

It may here be mentioned that in the maps which accompany this paper, the compound structure of the hot carbon flutings has been omitted, because the details are not, as a rule, seen in the spectra of heavenly bodies in which there are indications of carbon. The flutings are represented as simple ones beginning at the brightest edge and fading off gradually.

*Chemical Substances indicated by the Absorption Flutings and Bands.*

I may state that I have now obtained evidence to show that the origin of the following *absorption* flutings is probably as under:—

| No. of Fluting. | | Origin. | Wave-length of darkest most refrangible edge. | | Wave-length of less refrangible end, given by Dunér as measured in α Orionis. |
|---|---|---|---|---|---|
| 2 | ...... | Fe | 616 | ...... | 628 |
| 3 | ...... | Mn (2) | 585 | ...... | 595 |
| 4 | ...... | Mn (1)* | 558 | ...... | 564 |
| 5 | ...... | Pb (1)† | 544 | ...... | 550 |
| 6 | ...... | Ba‡ | 524 | ...... | 526 |
| 7 | ...... | Mg | 521 | ...... | 517 |
| 8 | ...... | Mg | 500 | ...... | 495 |

These flutings are characteristic of the whole class, and Dunér's catalogue consists chiefly of a statement of their presence or absence, or their varying intensities, in the different stars.

He gives other bands and wide lines which he has measured specially in α Orionis. I have also discovered the origin of the majority of these. They are as follows:—

| | | | Wave-length. |
|---|---|---|---|
| | I. | Fluting of Cr (1)...................... | 581 |
| | II. | ? ...................... | 570—577 |
| | III. | Fluting of Pb (2) ...................... | 567 |
| | IV. | ? ...................... | 543 |
| | V. | Line of Mn seen in bunsen.............. | 538—540 |
| | VI. | Band of Ba.......................... | 532—534 |
| Lines | 1. | Fluting of Cr (2)...................... | 559§ |
| | 2. | „ „ (3)...................... | 536 |
| | 3. | Line of Cr seen in bunsen ..... ....... | 520 |

* Means strongest fluting.

† The second Pb band has been seen in α Scorpii and α Orionis. Owing to an error in the map in the former paper, this fluting was ascribed to zinc.

‡ This is the second brightest band, wave-length 525. The first at wave-length 515 is masked by the radiation fluting at 516. See *post.*

§ *This is not given by Dunér.* It would be masked by the Mn fluting in the *star. I have inserted it to show* that we could not be dealing with the 3rd fluting *of Cr at 536 if we could* not explain the apparent absence of the 2nd.

| | | | |
|---|---|---|---|
| Lines | 4. | Ba band | 514* |
| | 5. | 1st, 2nd, and 3rd Ba flutings | 601 |
| | 6. | | 634 |
| | 7. | | 649 |

Band 1, which extends from wave-length 649·5 to 663·8, has not yet been allocated.

*Tests at our Disposal.*

In order to prove that my explanation of the nature of these celestial bodies is sufficient, a discussion of the individual observations of them, seeing that differences in the spectra are known to exist, should show that all the differences can be accounted for in the main by differences in the amount of interspace; that is to say, by a difference between the relative areas of space and meteorite in a section of the swarm at right angles to the line of sight. I say in the main, because subsequent inquiry may indicate that we should expect to find minor differences brought about by the beginnings of condensation in large as opposed to small swarms, and also by the actual or apparent magnitudes of the swarms varying their brilliancy, thus enabling a more minute study to be made of the same stage of heat in one swarm than in another.

How minor differences may arise will be at once seen when we consider the conditions of observation.

The apparent point of light generally seen is on my view produced not by a mass of vapour of more or less regular outline and structure, but by a swarm of meteorites perhaps with more than one point of condensation.

An equal amount of light received from the body may be produced by any stage, or number of nuclei, of condensation; and with any differences of area between the more luminous centre and the outliers of the swarm.

All these conditions producing light of very different qualities are integrated in the image on the slit of the spectroscope.

I have said "generally seen," because it has been long known that many of the objects I am now discussing are variable, as well as red, and that at the minimum they are not always seen as sharp points of light† but have been described as hazy.

The severe nature of the tests at our disposal will be recognised when we inquire what must follow from the variation of the spacing. Thus, as the spacing is reduced—

I. The temperature must increase.

* In the *early stages this band is* masked by the vivid light coming from the *carbon in the interspaces.*

† *Hind first noticed this in 1851.* Quoted by Arago, 'Astronomie Populaire.'

*α*. Vapours produced at the lowest temperatures will be the first to appear.

*β*. The spectrum of each substance must vary with the quantity of vapour produced as the temperature increases, and the new absorptions produced must be the same *and must follow in the same order* as those observed in laboratory experiments.

II. The carbon spectrum must first get more intense and then diminish afterwards as the spaces, now smaller, are occupied by vapours of other substances.

*α*. The longest spectrum will be that produced by mean spacing.

*β*. The masking of the dark bands by the bright ones must vary, and must be reduced as the mean spacing is reduced.

III. The continuous spectrum of the meteorites must increase.

*α*. There will be a gradually increasing dimming of the absorption bands from this cause.

*β*. This dimming will be entirely independent of the width of the band.

IV. The spectrum must gradually get richer in absorption bands.

*α*. Those produced at the lowest temperatures will be relatively widest first.

*β*. Those produced at the highest temperatures will be relatively widest last.

*γ*. They must all finally thin.

These necessary conditions, then, having to be fulfilled, I now proceed to discuss M. Dunér's individual observations. I shall show subsequently that there are, in all probability, other bodies besides those he has observed which really belong to this group.

## II. Discussion of Dunér's Individual Observations.

### *Consideration of the Extreme Conditions of Spacing.*

*Cæteris paribus*, when the interspaces are largest we should have a *preponderance* of the radiation of carbon, so far as quantity goes. The bands will be wide and pale, the complete radiation will not yet be developed; a minimum of metallic absorption phenomena—that is, only the flutings of magnesium (8 and 7), the first fluting of manganese (3), and the first fluting of iron (2); but the great width of the bright band at 517 will mask band 8.

When the interspaces are least, the radiation of carbon should give *place to the absorption* phenomena due to the presence of those *metallic vapours* produced at the highest temperature at which a *swarm can exist as such*; the bright flutings of carbon should be

diminished, and the true absorption flutings of Mg, Fe, Mn, Pb, and the band of Ba, should be enhanced in intensity.

There will be an *inversion* between the radiation and absorption.

The highest intensity of the absorption phenomena will be indicated by the strengthening of the bands 2, 3, 4, 5, and 6, and the appearance of the other flutings and bands specially recorded in α Orionis. The bands 7 and 8 will disappear as they are special to a low temperature, and will give way to the absorption of manganese, iron, *b*, &c.

This inversion, to deal with it in its broadest aspect, should give us at the beginning 7 strong, and 2, 3 weak, and at the end 7 and 8 weak, and 2, 3 strong.

The first stage, representing almost a cometic condition of the swarm before condensation has begun, has been observed in Nos. 3,* 23, 24, 25, 36, 68, 72, 81, 118, 247, 249. There is a very large number of similar instances to be found in the observations. The above are only given as examples.

The *last* stage, before all the bands fade away entirely, has been observed in Nos. 1, 2, 26, 32, 33, 38, 40, 61, 64, 69, 71, 75, 77, 82, 96, 101, 116. As before, these are only given as instances.

It is natural that these extreme points along the line of evolution represented in the bodies under consideration should form, as I think they do, the two most contrasted distinctions recorded by Dunér—that is, recorded in the greatest number of cases.

### *Origin of the Discontinuous Spectrum.*

I have already shown that when the meteorites are wide apart, though not at their widest, and there is no very marked condensation, the spectrum will extend farther into the blue, and therefore the flutings in the blue will be quite bright; in fact, under this condition the chief light in this part of the spectrum, almost indeed the only light, will come from the bright carbon. Under this same condition the temperature of the meteorites will not be very high, there will therefore be little continuous spectrum to be absorbed in the red and yellow. Hence we shall have discontinuity from one end of the spectrum to the other. This has also been recorded, and in fact it is the condition which gives us almost the most beautiful examples of the class (196, α Herculis, 141, 172, 229).

The *defect* of continuous light *in the blue* in this class, after condensation has commenced, and the carbon flutings are beginning to disappear, arises from defect of radiation of the meteorites, and hence in all fully-developed swarms the spectrum is not seen far into the blue for the reason that the vapours round each meteorite are at a temperature *such that fluting* absorption mainly takes place, although

* *The references are to the numbers* of the stars in Dunér's catalogue.

of course there must be some continuous absorption in the blue. Thi is perhaps the most highly-developed normal spectrum-giving condi tion; 44, 45, 55, 60, 65, 86, 92, 278 are examples.

*The Paling of the Flutings.*

Subsequently, the spectra are in all cases far from being discor tinuous, and the flutings, instead of being black, are pale. Thu while the bands are dark in the stars we have named, they are nc so dark in *α* Orionis. Here, in short, we have a great distinctio between this star and *α* Herculis, *o* Ceti, R Lyræ, and *ρ* Persei.

Obviously this arises from the fact that the average distanc between the meteorites have been reduced; their temperature bein thereby increased as more collisions are possible, the vapours a nearly as brilliant as the meteorites, and radiation from the inte spaces cloaks the evidences of absorption. Nor is this all: as th meteorites are nearer together, the area producing the bright flutin of the carbon is relatively reduced, and the bands 10 and 9 will fac for lack of contrast, while 8 and 7 will fade owing to the increase temperature of the system generally carrying the magnesium absor tion into the line stage; *b* is now predominant (see 102, 157, 16 114, 125, 135).

Under these conditions the *outer* absorbing metallic atmosphe round each meteorite will in all probability consist of Mn and F vapours, and in this condition the masking effect will least apply t them. This is so (114, 116); they remain dark, while the others a pale.

Here we have the indication of one of the penultimate stag already referred to.

*Phenomena of Condensation.*

Dealing specially with the question of condensation,—I hav already referred to possibly the first condition of all, recorded b Dunér in the observations now discussed—I may say that the fir real and obvious approach to it perhaps is observed when all, c nearly all, except 9 and 10 of the flutings are *wide* and *dark*. Th reasons will be obvious from what has been previously stated. Sti more condensation will give all, or nearly all, the bands wide an pale, while the final stage of condensation of the swarm will b reached when all the bands fade and give place to lines. We hav then reached Class II (107, 139, 168, 264); 2 and 3 should be an *are perhaps the* last to go (203).

*The Bands* 9 *and* 10.

With regard specially to the bands 9 and 10, which include between them a bright space which I contend is the second fluting of carbon, I may add that if this view is sound, the absence of 10 should mean a broad carbon band, and this is the condition of non-condensation, though not the initial condition. The red flutings should therefore be well marked—whether broad or not does not matter; but they should be dark and not *pale*. Similarly the absence of band 9 means non-condensation.

Therefore 9 and 10 should vary together, and as a matter of fact we find that their complete absence from the spectrum, while the metallic absorption is strong, is a very common condition (1, 2, 6, 16, 26, 32, 39, 40, 46, 54, 60).

That this explanation is probably the true one is shown by further consideration of what should happen to the red flutings when 9 and 10 are present. As the strong red flutings indicate condensation, according to my view this condensation (see *ante*) should pale the other flutings. This happens (3, 8, 13, 28, 35, 45, 30; and last, not least, among the examples, I give 50, $\alpha$ Orionis).

## III. Results of the Discussion.

*The Line of Evolution.*

I have gone over all the individual observations recorded by Dunér, and, dealing with them all to the best of my ability in the light afforded by the allocation of the bands to the various chemical substances, the history of the swarms he has observed seems to be as follows :—

(1) The swarm has arrived at the stage at which, owing to the gradual nearing of the meteorites, the hydrogen lines, which appeared at first in consequence of the great tenuity of the gases in the interspaces, give way to carbon. At first the fluting at 473 appears (as in many bright-line stars), and afterwards the one at 517. This is very nearly, but, as I shall show subsequently, not quite, the real beginning of the group, and the radiation is now accompanied by the fluting absorption of Mg, Fe, and Mn—bands 7, 2, 3. This is the absorption produced at the temperature of the oxy-coal-gas flame, while the stars above referred to give us the bright line of Mn seen at the temperature of the bunsen.

(2) The bright band of carbon at 517 narrows and unveils the Mg absorption at band 8. We have 8 now as well as 7 (both representing Mg), *added to the bands* 2 *and* 3, representing Fe and Mn, and *these latter now intensify.*

*(3) The spacing gets smaller*; the carbon, though reduced in

[redacted]



relative quantity, gets more intense. The second band at 473 in the blue gets brighter as well as the one at 517. We have now bands 9 and 10 added. This reduced spacing increases the number of collisions, so that Pb and Ba are added to Mg, Fe, and Mn. We have the bands 2, 3, 4, 5, 6, 7, 8, 9, and 10. This is the condition which gives, so to speak, the normal spectrum.

(4) This increased action will give us a bright atmosphere round each meteorite, only the light of the meteorite in the line of sight will be absorbed: we shall now have much continuous spectrum from the interspaces as well as the vapour of carbon. *The absorption flutings will pale*, and the Mg flutings will disappear on account of the higher temperature, while new ones will make their appearance.

(5) Greater nearness still will be followed by the further dimming of the bright carbon flutings including the one at 517. The blue end of the spectrum will shorten as the bands fade, narrow, and increase in number. If the star be bright, it will now put on the appearance of α Orionis; if dim, only the flutings of Fe and Mn (1), bands 2 and 3, will remain prominent.

(6) All the flutings and bands gradually thin, fade, and disappear. A star of the third group is the result.

In the latter higher-temperature stages we must expect hydrogen to be present, but it need not necessarily be visible, as the bright lines from the interspaces may cancel or mask the absorption in the line of sight of the light of the meteorites; but in case of any violent action, such as that produced by another swarm moving with great velocity, we must expect to see them bright, and they are shown bright in a magnificent photograph of ο Ceti, taken for the Draper Memorial, which I owe to the kindness of Professor Pickering. I shall return to this question.

*Stages antecedent to those recorded by Dunér.*

So far I have referred to the swarms observed by Dunér. The result of the discussion has been to show that all the phenomena are included in the hypothesis that the final stages we have considered are antecedent to the formation of stars of Group III, bodies which give an almost exclusively line absorption, though these bodies are probably not yet stars, if we use the term star to express complete volatilisation, similar to that observed in the case of our sun.

The question then arises, Are all the mixed fluting stages really included among the objects already considered?

It will be remembered that in my former communication I adduced evidence to the effect that the mixed fluting stage was preceded by *others in which the swarms* were still more dispersed, and at a lower *temperature. The first* condition gives us bright hydrogen; the last *little continuous spectrum* to be absorbed, so that the spectrum is one

with more bright lines than indications of absorption; and, in fact, the chief difference between the spectra of these swarms and of those still sparser ones which we call nebulæ lies in the fact that there are a few more bright metallic lines or remnants of flutings; those of magnesium, in the one case, being replaced by others of manganese and iron.

If my view be correct—if there are stages preceding those recorded by Dunér in which we get both dark and bright flutings—it is among bodies with spectra very similar to these that they should be found.

The first stage exhibited in the objects observed by Dunér is marked by flutings 7, 3, and 2 (omitting the less refrangible one not yet allocated), representing the flutings Mg, Mn, and Fe visible at the lowest temperatures.

The stars which I look upon as representing a prior stage should have recorded in their spectra the flutings 7 and 3 (without 2), representing Mg and Mn.

### *Classification into Species.*

We are now in a position to apply all that has gone before in summarised statements of the various spectral changes, including those connected with hydrogen, which take place not only in these objects studied by Dunér, but in those others to which I have referred as forming the true beginning of the group.

The following statements and tables, however, must not be taken as anything else than a first approximation to the real criteria of specific differences. I am convinced that further thought is required on them, and that such further thought will be well repaid.

### *The Sequence of the Various Bands in the Spectra of the Elements indicated by Bodies of the Group.*

In comparing the spectrum of an element which has been mapped in the laboratory with the absorption bands in the spectrum of a "star," we need only consider those bands and flutings which stand out prominently and are the first to flash out when there is only a small quantity present. Thus, in the flame spectrum of barium there is an almost continuous background of flutings with a few brighter bands in the green, and it is only important to consider the *bands*, as the flutings would mainly produce a general dimming of the continuous spectrum. In order to show at a glance what portions of the spectrum of an element it is most important for us to consider in this discussion, I have reconstructed the map of low-temperature spectra which I gave in my previous paper, with reference to those elements which are indicated in the spectra of bodies of Group II. Five orders of intensities are represented, the longest lines, flutings, or bands

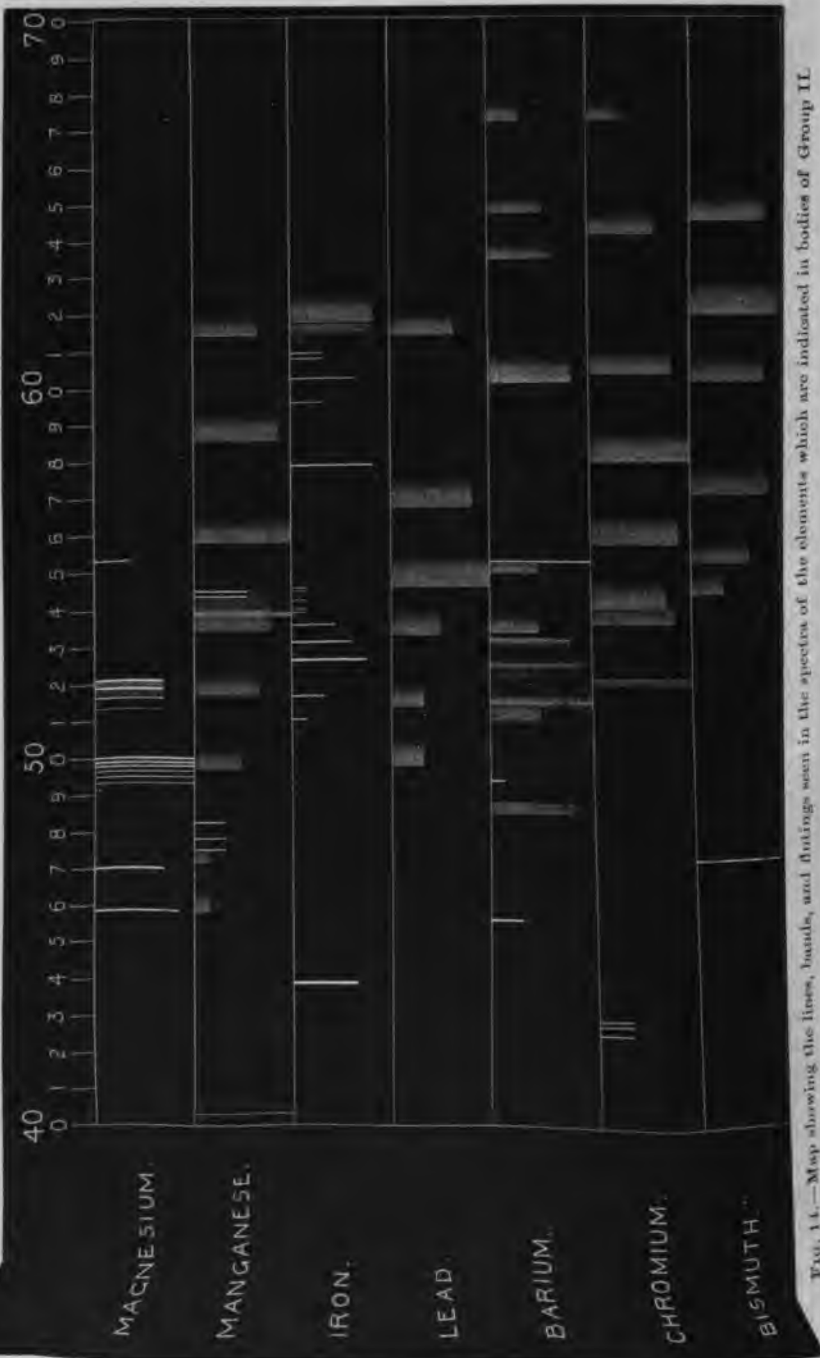



FIG. 14.—Map showing the lines, bands, and flutings seen in the spectra of the elements which are indicated in bodies of Group II.

he brightest (fig. 14). The lines, flutings, or bands in est horizon, in the case of each element, are those seen at est temperatures, and are the first to appear when only quantity of substance is present. Those in the upper are the faintest, and are only seen when the tempera- increased, or a considerable amount of the substance is ed. The map shows that if there are any indications of um, for instance, in bodies at low temperatures, the fluting rill be seen, possibly without the other fluting or lines. The ications of manganese will be the fluting at 558, and so on. on account of the masking effect of the spectrum of one element at of another, we may sometimes have an element indicated in pectrum, not by the brightest band or fluting in its spectrum, he second or even third in brightness; this, of course, only when the darkest band falls on one of the brightest flutings of or upon a dark band in the spectrum of some other element. ormer case the dark band will be cancelled or masked; in the ase the two absorptions will be added together, and form a and of a different shape.

*The Question of Masking.*

consider the masking effects of the bright carbon flutings e absorption spectrum of each of the elements which, accord- e results obtained, enter into the formation of Dunér's bands, the following as the main results:—

*esium.*—There are two flutings of magnesium to be considered, htest at 500 and the other at 521. In the earlier stages of stars only the fainter one at 521 is visible, but the absence rightest at 500 is accounted for by the masking effect of the arbon fluting starting at 517. As the carbon fades, the 517 narrows and the absorption of magnesium 500 becomes

*anese.*—The two chief flutings of manganese are at 558 and former being the brightest fluting in the spectrum. The uting is seen in all of Dunér's stars. The first fluting, 558, , does not appear as an absorption fluting until the radiation of carbon starting at 564 has narrowed sufficiently to unmask s thus easy to understand why, in some stars, there should be nd fluting of manganese without the first.

*m.*—The spectrum of barium consists of a set of flutings ex- the whole length of the spectrum, and standing out on this kground are three bright bands; the brightest band is at second is at 525, and the third, a broader band, is about 485. *nd band is recorded as* an absorption band in Dunér's stars, *rent absence of the first* band being due to the masking

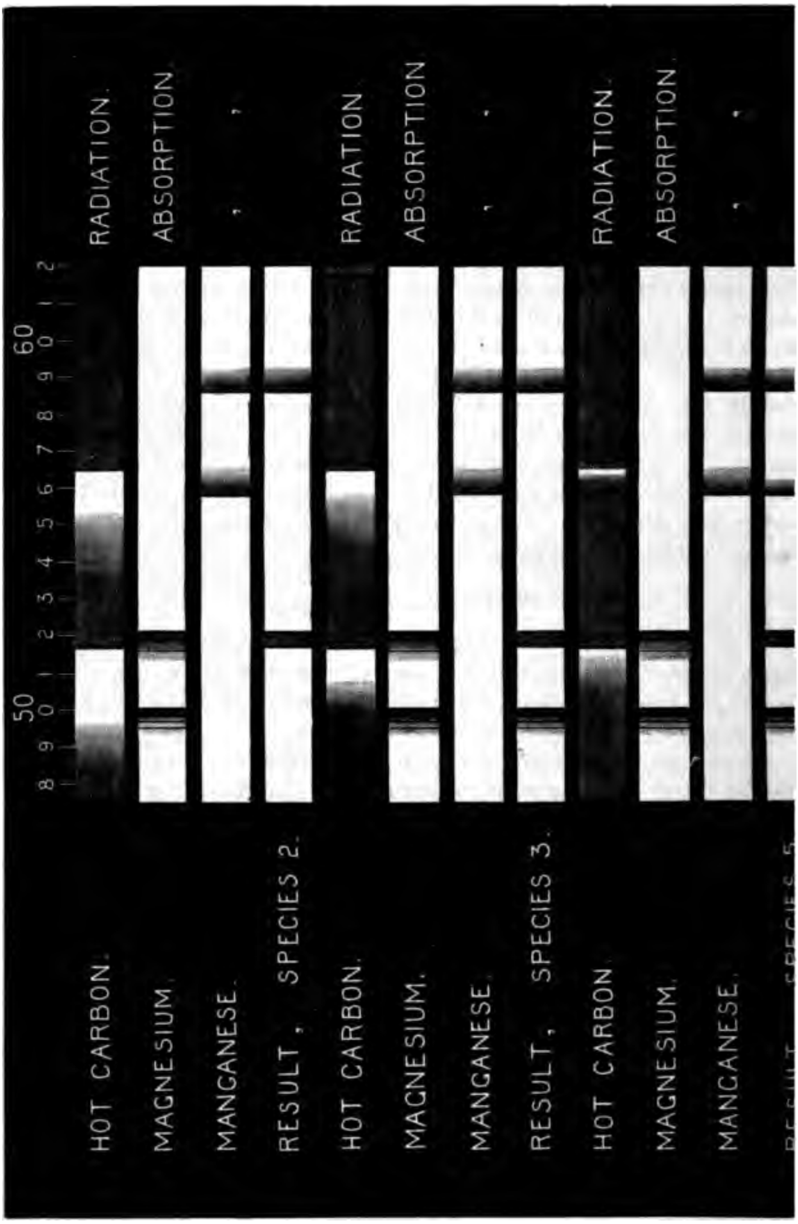



**Fig. 15.**—Diagram showing the effects of variations in width of the flutings of car *upon the integrated* spectra of carbon radiation and magnesium and mangan *absorption, as they* appear in different species of bodies of Group II.

effect of the bright carbon at 517. The third band at 485 probably forms a portion of band 9. A fourth band, at 533, and the three brightest flutings at 602, 635, and 648 are also seen in α Orionis.

*Chromium.*—The flutings of chromium do not form portions of the ten principal bands of Dunér, but the brightest are seen in α Orionis. The brightest fluting is at 580, and this forms band 1; the second, at 557, is masked by the manganese fluting at 558, and the third at 536 is seen as line 2. The chromium triplet about 520, which is visible in the bunsen, is seen as line 3.

*Bismuth.*—The brightest fluting of bismuth is at 620, the second is at 571, the third at 602, and the fourth is at 646. The first is masked by the iron fluting at 615, the second is seen in α Orionis as band 2 (570—577).

The points I consider as most firmly established are the masking effects of the bright carbon flutings and the possibility of the demonstration of the existence of some of the flutings in the spectrum by this means, if there were no other. There are two chief cases, the masking of the "nebula" fluting 500 by the bright carbon fluting with its brightest less refrangible edge at 517, and that of the strongest fluting of Mn = Mn (1) 558, by the other carbon fluting with its brightest edge at 564. I have little doubt that in some quarters my anxiety not to be content to refer to the second fluting of Mn without being able to explain the absence of the first one, will be considered thrown away, as it is so easy to ascribe any non-understood and therefore "abnormal" spectrum to unknown physical laws; but when a special research had shown me that at all temperatures at which the flutings of manganese are seen at all, the one at 558 retained its supremacy, I felt myself quite justified in ascribing its absence in species 1—4 to the cause I have assigned, the more especially as the Mg fluting which is visible even in the nebula followed suit.

### *The Characteristics of the Various Species.*

I append the following remarks and references to the number of the bodies in Dunér's catalogue, in which the specific differences come out most strongly, to the tabular statement. I also refer to some difficulties.

Sp. 1. The characteristic here is the almost cometary condition. All three bright carbon flutings generally seen in comets are visible; 474 standing out beyond the end of the dull blue continuous spectrum of the meteorites, 516 masking Mg 500, and 564 masking Mn(1) 558. The bands visible in the spectra of bodies belonging to this species will therefore be Mn(2) 586, and Mg(2) 521; band 9 will be so wide and pale that it would most likely escape detection. It is very doubtful whether any of the bodies the spectra of which have hitherto been recorded can be classed in this species, but laboratory

work assuredly points to their existence; it will therefore be extremely interesting if future observations result in their discovery. It is possible, however, that No. 150 of Dunér's list belongs to this species, but the details are insufficient to say with certainty. His description is as follows:—"150. Il me paraît y avoir une bande étroite dans le rouge, et une plus large dans le vert" (p. 55).

Sp. 2. Characteristics: appearance of Fe. The number of bands now visible is three—namely, 2, 3, and 7. The iron comes out as a result of the increased temperature. Mg(1) and Mn(1) are still masked by the bright carbon flutings, and there is still insufficient luminosity to make the apparent absorption-band 9 dark enough to be noticed.

Sp. 3. Characteristics: appearance of Mg 500, which has previously been masked by the carbon bright flutings 517. 7 and 8 are now the darkest band in the spectrum.

Sp. 4. Characteristics: appearance of Pb(1) 546, *i.e.*, band 5. This, if present in the earlier species at all, would be masked by the bright carbon at 564.

Sp. 5. Characteristics: Mn(1) is now unmasked. The bands now visible are 2, 3, 4, 5, 7, and 8, the two latter still being the widest and darkest, because they are essentially low-temperature phenomena.

Sp. 6. Characteristics: band 6, *i.e.*, Ba(2), 525, is now added. The first band of Ba at 515 is masked by the bright carbon at 517. The bands now visible are 2—8, 7 and 8 still being widest and darkest. They will all be pretty wide, and they will be dark because the continuous spectrum will be feebly developed.

Sp. 7. Characteristics: appearance of band 9. This, which has been already specially referred to, has been too wide and pale to be observed in the earlier species. Its present appearance is due to the narrowing and brightening of the carbon at 474 and the brightening of the continuous spectrum, the result being a greater contrast. Bands 7 and 8 still retain their supremacy, but all the bands will be moderately wide and dark.

Sp. 8. Characteristics: all the bands 2—9 are more prominent, so that 7 and 8 have almost lost their supremacy.

Sp. 9. Characteristics: appearance of band 1, the origin of which has not yet been determined. All the bands are well seen, and are moderately wide and dark.

Sp. 10. Characteristics: appearance of band 10, and in some cases 11. These become visible on account of the brightening of the carbon B fluting and the hydrocarbon fluting at 431. The spectrum is now at its greatest beauty, and is discontinuous.

Sp. 11. Characteristics: the bands are now becoming wider, and 2 and 3 are gaining in supremacy; 7 and 8 become narrower on

account of the increased temperature. 1 and 10 are only occasionally seen in this species.

Sp. 12. Characteristics: with the expansion of the continuous spectrum towards the blue, band 9 becomes very narrow, and cannot be observed with certainty. The other bands, with the exception of 7 and 8, are becoming wider and paler, while 2 and 3 still gain in supremacy.

Sp. 13. Characteristics: 9 has now entirely disappeared, 2 and 3 still retaining their supremacy.

Sp. 14. Characteristics: all the bands are pale and narrow; 2 and 3 will still be darkest, but the difference will not be so great as in the species preceding.

Sp. 15. Characteristics: in ordinary members of this group, 2 and 3 now alone remain visible: they are wide, but feeble, as the continuous spectrum which has been rapidly developing during the last changes is now strong.

Table A.—Specific Differences in Group II.

| Species. | Radiation flutings of carbon. | | | | | Absorption flutings. | | Dunér's bands. |
|---|---|---|---|---|---|---|---|---|
| | Hydro-carbon, 431. | Carbon B, 461. | Carbon A. | | | 10. | 9. | 8. |
| | | | 474. | 517. | 564. | | | Mg. |
| 1 | | | Very wide and pale | Wide and pale | Wide and pale | | | If present, masked by 517 |
| 2 | | | „ | „ | „ | | | „ |
| 3 | | | Narrowing and brightening | Narrowing and brightening | Narrowing and brightening | | | Appears dark |
| 4 | | | „ | „ | „ | | | Widens |
| 5 | | | „ | „ | Very narrow | | | „ |
| 6 | | | „ | Brighter and narrower | „ | | | Still darker and wider |
| 7 | | | „ | „ | „ | | Appears | „ |
| 8 | Very narrow | Very narrow | „ | „ | „ | | Darkens | Narrows |
| 9 | Widens | Widens | „ | „ | „ | | Strong | „ |
| 10 | „ | „ | Fading | Fading | „ | Appears | „ | „ |
| 11 | Narrows | Narrows | „ | „ | „ | Narrow in all but the brightest stars | Pales | Pales |
| 12 | Gone | „ | „ | Almost gone | „ | Disappears | Almost gone | „ |
| 13 | | Gone | „ | „ | „ | | Gone | „ |

Table A.—Specific Differences in Group II.

| Absorption flutings—*cont.* | | | | | | | |
|---|---|---|---|---|---|---|---|
| 7. | 6. | 5. | 4. | 3. | 2. | 1. | Whether hydrogen lines. |
| Mg. | Ba. | Pb(1). | Mn(1). | Mn(2). | Fe. | | |
| Thin and dark | | | Present, but masked by 564 | Thin and pale | Absent | | Yes |
| ” | | | ” | ” | Appears thin and pale | | No |
| ” | | | ” | ” | ” | | No |
| Darkens | | Appears dark | ” | ” | ” | | No |
| ” | | . . | Unmasked, dark | Darkens | Darkens | | No |
| Widens | Appears | Appears dark | . . | . . | . . | | No |
| ” | Darkens | Widens | Widens | Widens | Widens | | No |
| Narrows | ” | ” | ” | ” | ” | | No |
| ” | ” | ” | ” | ” | ” | Appears | Yes, bright and variable (possibly dark in α Orionis) |
| ” | . . | . . | ” | ” | ” | Still present | |
| Pales | Darkens | Pales | Pales | Now very broad and faint | Now very broad and faint | Fading | ? |
| ” | Pales | ” | ” | Pales | Pales | Gone | No |
| ” | ” | ” | ” | Wide and faint | Wide and faint | | No |
| Thin and faint | Thin and faint | Thin and faint | Thin and faint | Narrows | Narrows | | No |
| Gone | Gone | Gone | Gone | ” | ” | | No |

Table B.—Showing the Stars in Dunér's Catalogue arra
Species.

Species 1.

| No. of star. | Bands visible. |
|---|---|
| (150) | Narrow band in the red and a wider one in the green. |

Species 2.

| No. of star. | Bands visible. |
|---|---|
| (56) | 2, 3, 7. |
| (93) | 2, 3, 7; perhaps 4 and 5. |
| (220) | 2, 3, 7. |
| (233) | 2, 3, 7. |
| (246) | 2, 3, 7; possibly 4 and 5. Feebly developed. |

Species 3.

| No. of star. | Bands visible. |
|---|---|
| (42) | Bands weak; 2, 3, 7, 8 best visible. |
| (53) | 2, 3, 7, 8. |
| (70) | 2, 3, 7, 8; weak. |
| (185) | 2, 3, 7, 8. |
| (198) | 2, 3, 7, 8; narrow. |
| (228) | 2, 3; weak. 7 and 8 are well seen. |
| (276) | 2, 3, 7, 8; not very strong. |
| (290) | 2, 3, 7, 8. |

Species 4.

| No. of star. | Bands visible. |
|---|---|
| (7) | 2, 3, 5, 7, 8. |
| (95) | 2, 3, 7, 8; possibly also 4 and 5. |
| *(110)* | *2, 3, 7, 8;* narrow; 4 and 5 suspected. |

## Species 5.

| No. of star. | Bands visible. |
|---|---|
| (89) | 2, 3, 7, 8; 4 and 5 very weak. |
| (153) | 2, 3, and 7 wide; 4, 5, 8 pale. |
| (154) | 2, 3, 7, 8 narrow; 4 and 5 very narrow. |
| (173) | Feebly developed; the six ordinary bands feebly visible. |
| (253) | The six ordinary bands are plainly seen. |
| (258) | The six ordinary bands, but not very strong. |
| (267) | 2, 3, 7 well marked; 4, 5, 8 pale. |
| (271) | The six ordinary bands, feebly developed. |

## Species 6.

| No. of star. | Bands visible. |
|---|---|
| (6) | 2—8; wide and dark. |
| (19) | 2—8; 4 and 5 rather weak. |
| (39) | 2—8; strong and wide. |
| (48) | 2—8; well marked. |
| (67) | 2—8; wide and dark. |
| (74) | 2—8; wide and dark. |
| (76) | 2—8; well marked. |
| (83) | 2—8; wide and dark. |
| (99) | 2—8; well seen but not very strongly marked. |
| (188) | 2—8; wide and dark. |
| (189) | 2—8; wide and dark. |
| (194) | 2—8; wide but not very dark. |
| (202) | 2—8; wide and dark in the red and green-blue. |
| (208) | 2—8; well developed, especially in the blue-green. |
| (214) | 2—8; wide and dark. |
| (227) | 2—8; dark but narrow. |
| (247) | Bands plainly seen, but they are very pale, except 7 and 8. |
| (254) | 2—8; wide and dark. |
| (259) | 2—8; wide and dark, 7 and 8 strongest. |
| (260) | 2—8; dark, but not very wide. |
| (273) | 2—8; dark, but rather narrow. |
| (274) | There are seven bands, wide and rather dark. (I assume these to be 2—8.) |
| (285) | 2—8; well seen, not remarkably wide. |
| (289) | 2—8; very distinctly visible; 4 and 5 weak and narrow. |

## Species 7.

| No. of star. | Bands visible. |
|---|---|
| (24) | 2—9; pretty wide and dark, especially 7 and 8. |
| (97) | 2—9; *very dark*, rather narrow. |
| (115) | 2—9; wide, especially in the blue. |
| (143) | 2—9; wide and dark, especially in green-blue. |
| (181) | 2—9; very wide and dark, especially 7 and 8. |
| (195) | 2—9; 7 and 8 especially strong. |
| (229) | 2—9; very wide, but rather pale; 7 and 8 very wide and dark |
| (241) | 2—9; well seen. Those in green-blue wide and strong. |
| (249) | 7, 8, 9 are very wide and dark, others very narrow. |
| (252) | 2—10; wide and dark, especially in the blue. |
| (256) | 2—10 are seen. |
| (269) | 2—9; very dark, but not very wide. |
| (270) | 2—9; wide and dark, especially those in the blue. |
| (275) | 2—9; wide and dark, especially in the blue. |
| (284) | 2—9; wide and dark, especially in the green-blue. |

## Species 8.

| No. of star. | Bands visible. |
|---|---|
| (15) | 2—9; strongly developed, wide and dark. |
| (29) | 2—9; wide and dark. |
| (57) | 2—10; wide and dark. |
| (88) | 2—9; wide and strong. |
| (103) | 2—9; wide and dark. |
| (108) | 2—9; well marked. |
| (112) | 2—9; wide, dark. |
| (137) | 2—9; wide and dark. |
| (161) | 1—9; wide and dark throughout the spectrum. |
| (166) | 2—9; wide and dark, 4 and 5 darker than usual. |
| (184) | 2—9; wide and black, 6 rather weak. |
| (225) | 2—9; well seen throughout the spectrum. |
| (230) | 2—9; wide and rather dark. |
| (242) | 2—9 seen; *strong* and wide. |
| (251) | 2—9; wide and dark. |
| (263) | 2—9; wide and dark. |
| (278) | 2—9; wide and dark. |
| (283) | 2—9; wide and dark. |
| (286) | 2—9; wide and dark. |
| (291) | 2—9; wide and strong. |
| *(295)* | *2—9;* wide and dark, but spectrum is not very remarkable. |
| *(297)* | *2—9; well* marked, wide and dark. |

Species 9.

| No. of star. | Bands visible. |
|---|---|
| (9) | Bands wide and dark. |
| (12) | Bands wide and dark. |
| (20) | Bands wide and dark. |
| (23) | Bands very wide; those in the green-blue are dark. |
| (25) | 1—9; 7 and 8 darker than 2 and 3. |
| (37) | Some of the bands very wide; 7 and 8 strongest. |
| (44) | 1—9; very fine. |
| (65) | 1—9; wide and dark. |
| (66) | 1—9; very wide and dark; 6 well seen. |
| (118) | Bands wide and dark, especially in green-blue. |
| (123) | Bands wide and dark; full spectrum. |
| (148) | Bands wide and dark, even in the blue. |
| (156) | Band well marked and very wide throughout the whole spectrum. |
| (158) | Bands wide and dark, even in the blue. |
| (162) | 1—9; wide and dark. |
| (174) | Bands wide and dark. |
| (175) | Bands wide and dark. |
| (176) | Bands visible, even in the blue; not very dark. |
| (183) | 1—9; wide and dark. A narrow band between 3 and 4. |
| (186) | Bands well developed, even beyond the blue, but weak in red. |
| (204) | Bands wide and dark, even in the blue. |
| (216) | Bands wide and dark. |
| (217) | 1—9, including 6, are very wide and dark. |
| (221) | Bands wide and dark throughout the spectrum. |
| (237) | 2, 3, 7, 8 are strong; 1, 4, 5 well seen (6 and 9 are also most likely there). |
| (255) | Bands very dark and of extraordinary width. |
| (266) | 1—9; wide and dark. |
| (277) | 1—9; wide and dark. 4 and 5 wider than usual. |
| (281) | 1—9; wide and dark. |
| (293) | Bands wide and dark throughout the spectrum. |

## Species 10.

| No. of star. | Bands visible. |
|---|---|
| (4) (R Andromedæ) | Variable. |
| (18) | 1—11 inclusive. |
| (28) | Bands rather pale; like that of α Orionis. |
| (30) | Bands wide, both in green-blue and red. |
| (86) | 1—10; very wide and dark. |
| (91) | Bands very wide and dark, even in the blue. |
| (92) | 1—10; very wide and dark. |
| (131) | 1—10; 2 and 3 wide, others relatively narrow. |
| (141) | 1—10; very wide and dark. |
| (172) | 2—10, possibly 1; wide and dark. |
| (196) | 1—10; very wide and black. |
| (232) | 1—10. |
| (239) | 1—10; very fine. |

## Species 11.

| No. of star. | Bands visible. |
|---|---|
| (5) | 2—9; 3 is very wide. |
| (55) | 2—9; fine. |
| (87) | 2—9; wide and dark, especially 2 and 3. |
| (98) | 2—9; wide and visible, even in the blue; rather pale. |
| (135) | 1—9; wide and pale. |
| (149) | 1—9; wide and very *dark*. Bands in the red fine. |
| (152) | 1—9; well marked, fine in the red. |
| (171) | 2—9; 2 and 3 strongest. |
| (177) | 2—9; strong and wide, especially in the red. |
| (191) | 2—9; wide and dark, especially 2 and 3. |
| (193) | 2—9; 2 and 3 strongly marked. |
| (197) | 2—9; wide. |
| (199) | 2—9; very wide and *dark*, especially in the red. 4 and 5 are wider than usual. |
| (212) | 2—9; wide and dark. 2 and 3 are the strongest. |
| (218) | Bands wide, but not very dark, as far as 9. |
| (234) | 2—9; wide. |
| (245) | Bands wide, but pale. Strongest in the red. |
| (288) | Bands wide and pale, but visible even in the blue. |

Species 12.

| No. of star. | Bands visible. |
|---|---|
| (27) | 2—8; wide and pale. |
| (46) | 2—8, possibly 9. |
| (51) | 2—8, possibly 9. |
| (52) | 2—8, possibly also 9; wide, but not very dark. |
| (60) | 2—8, possibly 9; wide and dark. |
| (78) | Bands visible even in the blue; wide but pale. |
| (117) | 2—8; feebly developed. |
| (122) | 2—8; wide, but rather pale. |
| (126) | 2—8, possibly 9; 2 and 3 strong. |
| (129) | 2—8; wide and pale. |
| (133) | Bands wide and dark, especially in the red. |
| (164) | 2—8, probably also 9; red bands darkest. |
| (215) | 2—8; not very strong. |
| (264) | 2—8, possibly 9; wide, but not very dark. |

## Species 13.

| No. of star. | Bands visible. |
|---|---|
| (1) | 2—8; red bands strongest. |
| (2) | 2—8; 2 and 3 strongest. |
| (16) | 2 and 3, pretty strong; 4—8, wide and pale. |
| (17) | 2—8; 2 and 3 strongest. |
| (26) | 2—8; 2 and 3 strongest. |
| (32) | 2—8; 2 and 3 strongest. |
| (33) | 2—8; 2 and 3 strongest. |
| (36) | 2—8; 2 and 3 terminated by strong lines. *b* present. |
| (38) | 2—8; 2 and 3 strongest. |
| (40) | 2—8; 2 and 3 strongest. |
| (54) | 2—8; 2 and 3 strongest. |
| (61) | 2—8; 2 and 3 strongest. |
| (62) | Red bands fairly strong; 7 and 8 weak; 4 and 5 narrow. |
| (64) | 2—8; 2 and 3 strong. |
| (69) | 2—8; 2 and 3 very dark. |
| (71) | 2—8; 2 and 3 strong. |
| (75) | 2—8; wide and dark, especially in the red. |
| (82) | 2—8; all strong, but especially 2 and 3. |
| (104) | 2 and 3 strong and wide, 7 and 8 fairly strong, 4 and 5 weak. |
| (109) | 2—8; wide and dark, especially in the red. |
| (116) | 2—8; very pale, except 2 and 3. |
| (120) | 2—8; well seen, 2 and 3 widest. |
| (121) | 2—8; 2, 3, 7 strongest. |
| (124) | 2—8; 2 and 3 especially wide and dark. |
| (130) | 2—8; well seen, 2 and 3 strong. |
| (132) | 2—8; narrow, except 2 and 3. |
| (144) | 2—8; well seen, 2 and 3 strongest. |
| (145) | 2—8; well seen, 2 and 3 strongest. |
| (146) | 2—8; rather narrow, 2 and 3 widest. |
| (155) | 2—8; 2 and 3 strong, but not very wide. |
| (160) | 2, 3, 4, 5, 7, 8; 2 and 3 wide and dark. |
| (182) | 2—8; 2 and 3 strongest. |
| (200) | 2—8; well seen, 2 and 3 are the strongest. |
| (203) | 2—8; seen with difficulty, 2 and 3 strongest. |
| (205) | 2—8 are visible, 2 and 3 darkest. |
| (207) | 2—8; 2 and 3 strongest. |
| (211) | 2—8; red strongest. |
| (240) | The six ordinary bands are strong, but only those in the red wide. |
| (243) | The six ordinary bands; wide and dark in the red; 4 and 5 n |
| (244) | 2 and 3; rather wide. Also 7 and 8 seen (not well marked). |
| (268) | 2 and 3 wide and dark; 7 and 8 rather narrow; 4 and 5 easily seen. |
| (280) | Six bands, strongest in the red. |
| *(287)* | *2 and 3* wide and strongly marked; the others not so strong. |
| *(292)* | *The six* ordinary bands are visible, widest in the red. |
| *(294)* | *2—8; 2, 3* strong, the others narrow. |

## Species 14.

| No. of star. | Bands visible. |
|---|---|
| (22) | 2—8 are seen, but they are not well marked. |
| (49) | 2—8 ; narrow and not very dark. |
| (90) | 2—8 ; narrow and not very dark. |
| (94) | 2—8 ; not strongly marked ; 4 and 5 weak. |
| (107) | 2—8 ; very narrow. |
| (111)* | 2—9 ; narrow. |
| (113) | 2—8 ; feebly developed. |
| (138) | 2—8 ; not strongly marked. 4 and 5 are very narrow. |
| (140) | 2, 3, 5, 7, 8 ; pale and narrow, feebly developed. |
| (142) | 2—8 ; not very wide. |
| (167) | 2—8 ; narrow and not very dark. |
| (169) | 2—8 ; narrow. |
| (179) | 2—8 ; narrow and not very dark. |
| (180) | 2—8 ; narrow. |
| (187) | 2—8 ; weak. |
| (250) | Bands plain, but neither wide nor dark. |
| (282) | The six ordinary bands, but only 2, 3, and 7 are passably wide. |

* In this case the carbon has not died out as early as it usually does, so that band 9 is seen in addition to 2—8.

## Species 15.

| No. of star. | Bands visible. |
|---|---|
| (41) | 2 and 3 wide and dark, others feeble and narrow. |
| (50)* α Orionis. | 1—10 ; rather pale and narrow. |
| (96) | Bands very narrow ; 2 and 3 strongest. |
| (101) | 2 and 3 very well seen, 7 and 8 weak, 4 and 5 doubtful. |
| (136) | Bands in the red are wide, the others narrow. |
| (139) | Bands weak and narrow. Something like the spectrum of Aldebaran. |
| (147) | 2, 3, 7 ; others extremely narrow. |
| (190) | 2, 3, 7, narrow bands ; the rest almost like lines. |
| (226) | Feebly developed, 2 and 3 strongest. |
| (235) | Bands neither wide nor dark ; feebly developed. |
| (265) | Bands plainly seen, but extremely narrow. |
| (279) | 2, 3, 7 dark, not very wide ; 4 and 5 narrow. |

* The additional bands seen in this "star" are in all probability due to its great brilliancy as compared with other *members* of the group.

## Indefinite—Early Stages.

| No. of star. | Bands visible. |
|---|---|
| (3) | Bands weak, but very wide, especially in the green and blue. |
| (11) | Bands wide, especially in the green and blue. |
| (21) | Bands wide and dark, especially in the green and blue. |
| (34) | Bands dark, but rather narrow. |
| (45) | Bands wide; those in the blue are stronger than those in the |
| (59) | Fairly well developed; 4 and 5 narrow. |
| (68) | Bands wide and dark, especially in the green and blue. |
| (72) | Feebly developed; bands widest in green and blue. |
| (81) | Feebly developed; 7 and 8 are best visible. |
| (100) | Bands wide and dark, especially 7 and 8. |
| (106) | Bands dark, and wide in the blue and green. |
| (134) | Bands wide and dark, especially in green and blue. |
| (151) | Bands wide and dark, especially in the green and blue. |
| (159) | Bands in blue and green are very wide and dark. |
| (165) | Bands wide and well seen, especially in green and blue. |
| (168) | Bands wide and strong, especially in the green and blue. |
| (170) | The bands in the blue are very wide. |
| (192) | Bands are wide, especially in the green and blue. |
| (201) | Bands wide and well seen, especially 7 and 8. |
| (206) | Bands easily seen in green and blue; feebly developed. |
| (209) | Bands well seen, especially in green and blue. |
| (222) | Bands wide and dark, especially in green and blue. |
| (223) | Bands visible throughout the spectrum, strongest in green an |
| (224) | Bands in green and blue are very wide and dark. |
| (248) | Bands dark and visible even in the blue. |
| (262) | Bands visible even in the blue, weakest in the red. |

## Indefinite—Later Stages.

| ar. | Bands visible. |
|---|---|
| | Bands pretty wide, and visible even in blue. |
| | Bands enormously wide. |
| | Bands narrow and dark throughout the spectrum, but especially in the red. |
| | Feebly developed, but the bands seem to be wide. |
| | Bands enormously wide, but very feeble. |
| | Bands wide, spectrum weak. |
| | Bands wide and dark in the red, weaker in the blue and green. |
| | Bands wide, but not very dark; seen in blue also. |
| | Feebly developed, but 2—8 are seen (Dunér's "feebly developed" means much developed from my point of view, if the bands are thin). |
| | Bands wide, but pale. |
| | Bands wide and pale, except 2 and 3, which are strong. |
| | Bands wide throughout the spectrum. |
| | Bands wide and pale, but visible even in the blue. |
| | Bands wide, but very pale. |
| | Bands wide, but pale. |
| | Bands are pale, but visible even in the blue. |
| | Bands wide, but feeble. |
| | Bands in the red well marked; 4 and 5 weaker. |
| | The six ordinary bands are seen, but they are rather pale. |
| | Bands not very dark, but wide and visible even in the blue. |
| | Bands wide, but weak. |

## 'otally Indeterminate, on account of Absence of Details.

| ar. | Bands visible. |
|---|---|
| | Feebly developed. (No details given.) |
| | Feebly developed. |
| | Feebly developed; bands very indistinct. |
| | Doubtful whether III*a* or III*b*. |
| | Only recognised as III*a* on one occasion. |
| | Feebly developed. |
| | Doubtful whether III*a* or III*b*. |
| | Feebly developed; somewhat uncertain. |
| | Very feebly developed. |
| | Feebly developed. |
| | Feebly developed. |
| | Very feebly developed. |
| | Very feebly developed. |
| | Not well marked. |
| | *? IIIa.* |

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## Part V.—On the Cause of Variation in the Light of Bodies of Groups I and II.

### I. General Views on Variability.

In my former paper I referred to the collision of meteor-swarms as producing "new stars," and to the periastron passage of one swarm through another as producing the more or less regular variability observed in the case of some stars of the group under consideration.

I propose now to consider this question of variability at somewhat greater length, but only that part of it which touches non-condensed swarms; *i.e.*, I shall for the present leave the phenomena of new stars, and of those whose variability is caused by eclipses, aside.

It is not necessary that I should pause here to state at length the causes of stellar variability which have been suggested from time to time. It will suffice, perhaps, that I should refer to one of the first suggestions which we owe to Sir I. Newton, and to the last general discussion of the matter, which we owe to Zöllner ('Photometrische Untersuchungen,' 76 and 77, p. 252).

Newton ascribed that special class of variability, to which I shall have most to refer in the sequel, as due to the appulse of comets.

"Sic etiam stellæ fixæ, quæ paulatim expirant in lucem et vapores, cometis in ipsas incidentibus refici possunt, et *novo alimento accensæ pro stellis novis haberi.* Hujus generis sunt stellæ fixæ, quæ subito apparent, et sub initio quam maxime splendent, et subinde paulatim evanescunt. Talis fuit stella in cathedra Cassiopeiæ quam Cornelius Gemma octavo Novembris 1572 lustrando illam cœli partem nocte serena minime vidit; at nocte proxima (Novem. 9) vidit fixis omnibus splendidiorem, et luce sua vix cedentem Veneri. Hanc Tycho Brahæus vidit undecimo ejusdem mensis ubi maxime splenduit; et ex eo tempore paulatim decrescentem et spatio mensium sexdecim evanescentem observavit" ('Principia,' p. 525, Glasgow, 1871).

With regard to another class of variables he makes a suggestion which has generally been accepted since:—

"Sed fixæ, quæ per vices apparent et evanescunt, quæque paulatim crescunt, et luce sua fixas tertiæ magnitudinis vix unquam superant, videntur esse generis alterius, et revolvendo partem lucidam et partem obscuram per vices ostendere. Vapores autem, qui ex sole et stellis fixis et caudis cometarum oriuntur, incidere possunt per gravitatem suam in atmosphæras planetarum et ibi condensari et converti in *aquam et spiritus* humidos, et subinde per lentum calorem in *sales et sulphura et tincturas* et limum et lutum et argillam et arenam et *lapides et coralla et* substantias alias terrestres paulatim migrare."

Zöllner in point of fact advances very little beyond the views advocated by Newton and Sir W. Herschel. He considers the main causes of variability to be as follows. He lays the greatest stress upon an advanced stage of cooling, and the consequent formation of scoriæ which float about on the molten mass. Those formed at the poles are driven towards the equator by the centrifugal force, and by the increasing rapidity of rotation they are compelled to deviate from their course. These facts, and the meeting which takes place between the molten matter, flowing in an opposite direction, influence the form and position of the cold non-luminous matter, and hence vary the rotational effects, and therefore the luminous or non-luminous appearance of the body to distant observers.

This general theory, however, does not exclude other causes, such as, for instance, the sudden illumination of a star by the heat produced by collision of two dark bodies, variability produced by the revolution of a dark body, or by the passage of the light through nebulous light-absorbing masses.

If the views I have put forward are true, the objects now under consideration are those in the heavens which are least condensed. In this point, then, they differ essentially from all true stars like the sun.

This fundamental difference of structure should be revealed in the phenomena of variability; that is to say—The variability of the bodies we are now considering should be different in *kind* as well as in degree from that observed in some cases in bodies like the sun or α Lyræ, taken as representing highly condensed types. There is also little doubt I think, that future research will show that when we get short-period variability in bodies like these, we are here really dealing with the variability of a close companion.

## II. On the Variability in Group I.

That many of the nebulæ are variable is well known, though so far as I am aware there are no complete records of the spectroscopic result of the variability. But bearing in mind that in some of these bodies we have the olivine line by itself, and in others, which are usually brighter, we have the lines of hydrogen added, it does not seem unreasonable to suppose that any increase of temperature brought about by the increased number of collisions should add the lines of hydrogen to the spectrum of a nebula in which they were not previously visible.

The explanation of the hydrogen in the variable *stars* is not at first so obvious, but a little consideration will show that this must happen if *my theory be true.*

*Since the stars with bright lines* are, as I have attempted to show,

very akin to nebulæ in their structure, we might, reasoning by analogy, suppose that any marked variability in their case also would be accompanied by the coming out of the bright hydrogen lines.

This is really exactly what happens both in $\beta$ Lyræ and in $\gamma$ Cassiopeiæ. In $\beta$ Lyræ the appearance of the lines of hydrogen has a period of between six and seven days, and in $\gamma$ Cassiopeiæ they appear from time to time, although the period has not yet been determined.

### III. On the Variability in Group II.

This same kind of variability takes place in stars with the bright flutings of carbon indicated in their spectra, $o$ Ceti being a marvellous

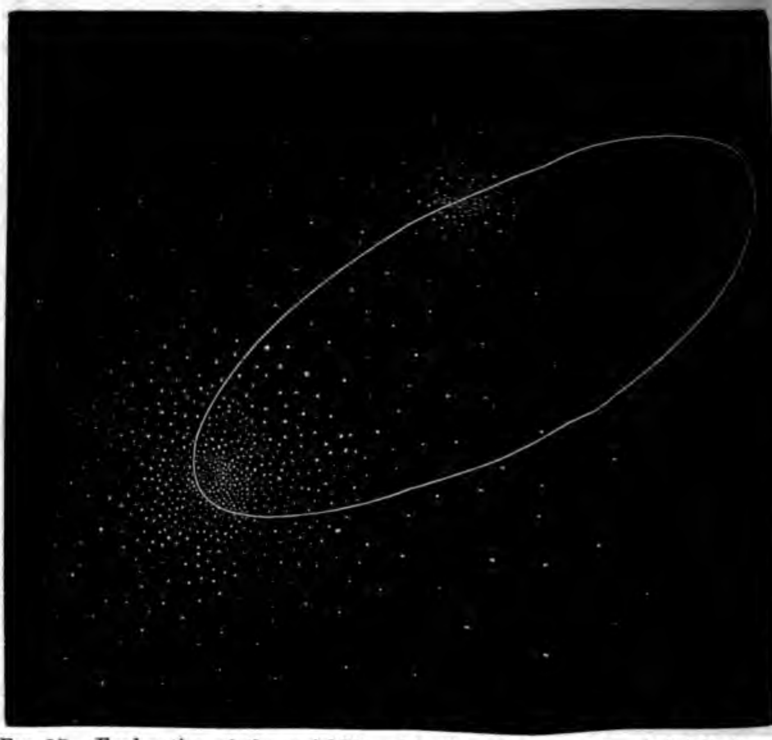
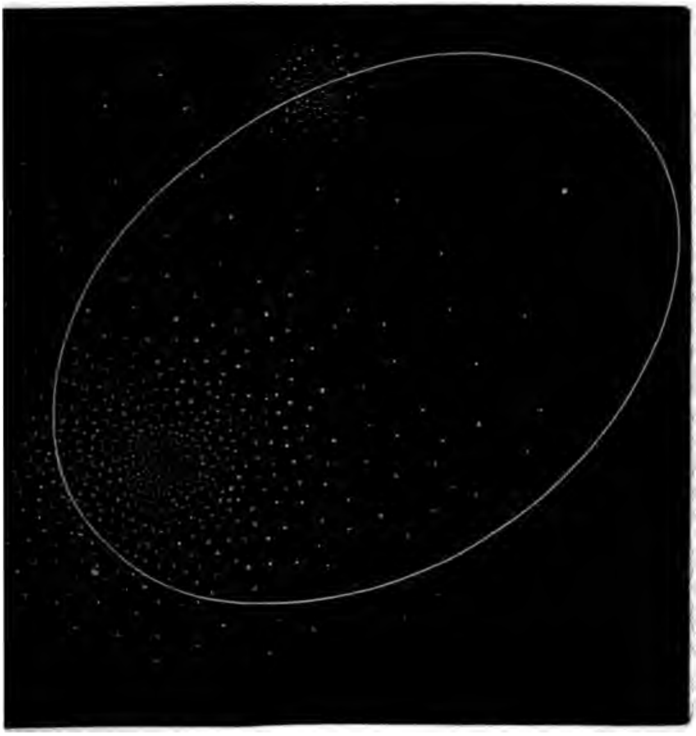


Fig. 17.—Explanation of the variability of bodies of Group II. (1.) Maximum variation. The ellipse represents the orbit of the smaller swarm, which revolves round the larger. The orbit of the revolving swarm is very elliptical, so that at periastron the number of collisions is enormously increased.

case in point. In $\alpha$ Orionis, one of the most highly developed of these stars, the hydrogen lines are invisible; the simple and sufficient explanation of this being that, as I have already suggested, the bright lines from the interspaces now at their minimum and containing

's at a very high temperature—*teste* the line-absorption spectrum ginning to replace the flutings—balance the absorption of the ic nuclei.

hing which in this condition of light-equilibrium will increase ount of incandescent gas and vapour in the interspaces will bout the appearance of the hydrogen lines as bright ones. ing above all things most capable of doing this in a most idental fashion is the invasion of one part of the swarm by



-Explanation of the variability of bodies of Group II. (2.) Medium tion. In this case, there will be a greater number of collisions at perias- than at other parts of the orbit. The variation in the light, however, will be very great under the conditions represented, as the revolving swarm never very near the middle of the central one.

: one moving with a high velocity. This is exactly what I te. The wonderful thing under these circumstances then be that bright hydrogen should *not* add itself to the bright , not only in bright-line stars, but in those the spectrum of which s of mixed flutings, bright carbon representing the radiation. *propose to use this question* of variability in Group II as a *test of my views.*

The first test we have of the theory is that there should variability in this group than in any of the others. Othe follows: (2) When the swarm is most spaced, we shall have results from collisions, but (3) when it is fairly condensed, at periastron passage (if we take the simplest case of a doub *posse*) will be greatest of all, because (4) condensation may bring the central swarm almost entirely within the orl secondary (cometic) body, in which case no collisions could

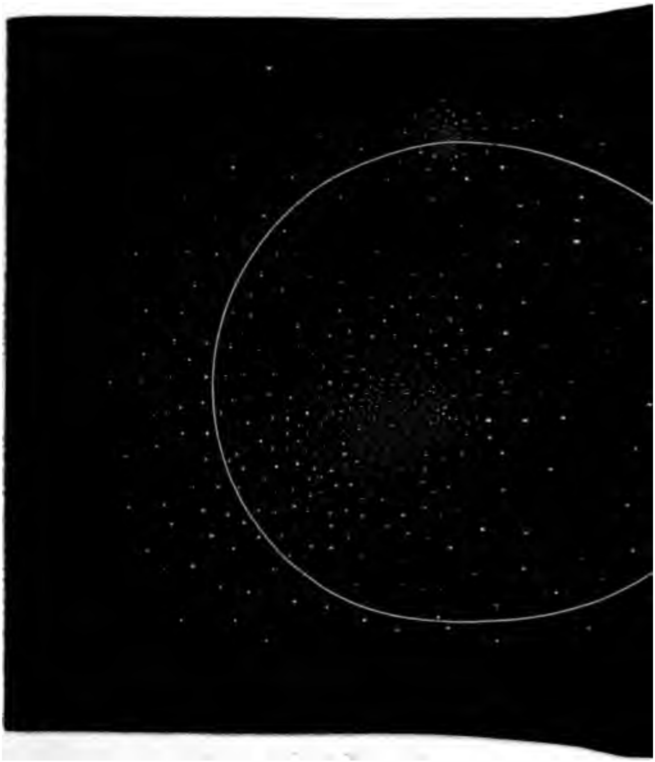


FIG. 19.—Explanation of the variability of bodies of Group II. (3., variation. Under the conditions shown, the smaller swarm wil entirely out of the larger one, and at periastron the number of coi not be very greatly increased. Consequently, the variation in the light given out will be small.

In the light of what has gone before it is as easy to te points as the former ones.

*The Frequent Occurrence of Variability in Group II.*

The total number of stars included in Argelander's C which deals generally with stars down to the ninth magnitu

which, however, are many stars between the ninth and tenth, is 324,118. The most complete catalogue of variables (without distinction) that we have has been compiled by Mr. Gore, and published in the 'Proceedings of the Royal Irish Academy' (series ii, vol. 4, No. 2, July, 1884, pp. 150—163). I find 191 known variables are given; of these 111 are in the northern hemisphere and 80 in the southern hemisphere.

In the catalogue of *suspected* variable stars given in No. 3 of the same volume (January, 1885, pp. 271—310), I find 736 stars, of which 381 are in the northern and 355 in the southern hemisphere.

Taking, then, those in the northern hemisphere, both known and suspected, we have the number 492.

We have then as a rough estimate for the northern heavens one variable to 659 stars taken generally.

The number of objects of Group II observed by Dunér, and recorded in his admirable memoir, is 297; of these forty-four are variable.

So that here we pass from 1 in 657 to 1 in 7.

Of the great development of variability-conditions in this group then there can be no question.

To apply the other tests above referred to, I have made a special study of the observations of each variable recorded by Dunér. I find they may be grouped in the following

*Table of Variables.*

1. All bands visible but narrow.

| No. in Dunér Cat. | Name. | Max. | Min. | Period. | |
|---|---|---|---|---|---|
| 269 | μ Cephei ........ | 4 ? | 5 ? | irreg. | |

2. Bands well marked, but feebler in Red.

| No. in Dunér Cat. | Name. | Max. | Min. | Period. | |
|---|---|---|---|---|---|
| 186 | W Herculis (? V).. | >8 | <12 | 290 ? | |
| 222 | R Sagittarii ...... | 7 | 12 | 270 | |
| 81 | S Hydræ......... | 7·8 | <12 | 256 | |

3. Bands wide and strong, especially 7 and 8.

| No. in Dunér Cat. | Name. | Max. | Min. | Period. | |
|---|---|---|---|---|---|
| 23 | T Arietis ........ | 8 | 9—10 | 324 | |
| 37 | R Tauri.......... | 7·8 | < 13 | 326 | |
| 68 | S Canis Min. ..... | 7 | < 11 | 332 | |
| 76 | R Cancri......... | 6 | < 11—12 | 360 | |
| 91 | R Leonis Min..... | 5 | 10 | 313 | |
| 100 | R Urs. Maj....... | 6 | 12 | 303 | |
| 106 | R Crateris........ | > 8 | < 9 | 160 ? | |
| 118 | R Corvi.......... | 7 | < 11 - 13 | 319 | |
| 159 | R Boötis.......... | 6 | 12 | 223 | |
| 165 | S Libræ.......... | 8 | 12—13 | 190 ? | |
| 170 | R Serpentis ...... | 5·6 | < 11 | 358 | |
| 181 | U Herculis ...... | 6·7 | 11—12 | 408 | |
| 192 | S Herculis ...... | 6 | 12 | 303 | |
| 195 | R Ophiuchi ...... | 7·8 | 12 | 302 | |

4. All bands markedly wide and strong.

| No. in Dunér Cat. | Name. | Max. | Min. | Period. | |
|---|---|---|---|---|---|
| 18 | *o* Ceti ........... | 2—5 | 8—9 | (331) | |
| 20 | R Ceti ........... | 8 | < 13 ? | 167 | |
| 29 | *ρ* Persei ......... | 3·4 | 4·2 | irreg. | Many lines. |
| 92 | R Leonis ......... | 5 | 10 | 313 | |
| 141 | R Hydræ......... | 4·5 | 40 ? | (437) | |
| 158 | V Boötis ......... | .. | .. | .. | |
| 166 | S Coronæ ........ | 6 | 12 | 361 | |
| 184 | *g* Herculis........ | 5 | 6 | irreg. | |
| 196 | *a* Herculis........ | 3 | 4 | irreg. | |
| 217 | R Lyræ .......... | 4·3 | 4·6 | 46 | |
| 221 | R Aquilæ ........ | 6·7 | 11 | 345 | |
| 239 | *χ* Cygni .......... | 4 | 13 | 406 | |
| 293 | R Aquarii .... .. | 6 | 11 | 388 | |

5. Bands wide, but pale.

| Name. | Max. | Min. | Period. | |
|---|---|---|---|---|
| T Cassiopeiæ ...... | 6·7 | 11 | 436 | |
| T Urs. Maj. ...... | 7 | 12 | 256 | |
| R Virginis ....... | 6·7 | 11 | 146 | |
| R Camel. ........ | 8 | 12? | 266 | |
| R Cygni.......... | 6 | 13 | 425 | |
| β Pegasi.......... | 7 | 12 | 382 | |
| T Herculis ....... | 7 | 12 | 165 | |
| R Androm. ....... | 5·6 | < 12—13 | 405 | |

6. Bands thin and pale.

| Name. | Max. | Min. | Period. | |
|---|---|---|---|---|
| α Orionis ........ | 1 | 1·4 | irreg. | |
| S Urs. Maj. ...... | 7·8 | 11 | 225 | |
| R Draconis ....... | 6·7 | 11—12 | 247 | |
| S Vulpec. ........ | | | | |
| R Vulpec......... | 7·8 | 13 | 137 | |

ance at the above tables will show that the kind of variability ted by these objects is a very special one, and is remarkable for at range. The light may be stated in the most general terms to bout six magnitudes—from the sixth to the twelfth. This, I is a fair average; the small number of cases with a smaller on I shall refer to afterwards. A variation of six magnitudes roughly that the variable at its maximum is somewhere about nes brighter than at its minimum.*

ve already indicated that, with regard to the various origins of riability of stars which have been suggested, those which have lways most in vogue consider the maximum luminosity of the the normal one; and, indeed, with regard to the Algol type of

ained by the formula $L_m = (2·512)^n \,.\, L_{m+n}$.
: differences of 5, 6, 7 and 8 mag. we get

$$L_m = 100·02 \,.\, L_{m+5}$$
$$= 251·24 \,.\, L_{m+6}$$
$$= 631·11 \,.\, L_{m+7}$$
$$= 1585·35 \,.\, L_{m+8}$$

$L_m$ = light of a star of magnitude $m$.
$L_{m+n}$ = " " $n$ magnitudes fainter.

stars of short period, which obviously are not here in question, there can be no reasonable doubt, that the eclipse explanation is a valid one; but in cases such as we are now considering, when we may say that the ordinary period is a year, this explanation is as much out of place on account of period, as are such suggested causes as stellar rotation and varying amount of spotted area on a stellar surface, on account of range.

We are driven, then, to consider a condition of things in which the minimum represents the constant condition, and the maximum a condition imposed by some cause which produces an excess of light; so far as I know the only explanation on such a basis as this that has been previously offered is the one we owe to Newton, who suggested such stellar variability as that we are now considering was due to conflagrations brought about at the maximum by the appulse of comets.

*How the Difficulty of Regular Variability on Newton's View is got over in mine.*

It will have been noticed that the suggestion put forward by myself is obviously very near akin to the one put forward by Newton, and no doubt his would have been more thoroughly considered than it has been hitherto, if for a moment the true nature of the special class of bodies we are now considering had been *en évidence.* We know that some of them at their minimum put on a special appearance of their own in that haziness to which I have before referred as having been observed by Mr. Hind. My researches show that they are probably nebulous, if indeed they are not all of them planetary nebulæ in a further stage of condensation, and such a disturbance as the one I have suggested would be certain to be competent to increase the luminous radiations of such a congeries to the extent indicated.

Some writers have objected to Newton's hypothesis on the ground that such a conflagration as he pictured could not occur periodically; but this objection I imagine chiefly depended upon the idea that the conflagration brought about by one impact of this kind would be quite sufficient to destroy one or both bodies, and thus put an end to any possibilities of rhythmically recurrent action. It was understood that the body conflagrated was solid like our earth. However valid this objection might be as urged against Newton's view, it cannot apply to mine, because in such a swarm as I have suggested, an increase of light to the extent required might easily be produced by the incandescence of a few hundred tons of meteorites.

I have already referred to the fact that the initial species of the stars we are now considering have spectra almost cometary, and this leads us to the view that we may have among them in some cases swarms

with double nuclei—incipient double stars, a smaller swarm revolving round the larger condensation, or rather round their common centre of gravity. In such a condition of things as this, it is obvious that, as before stated, in the swarms having a mean condensation this action is the more likely to take place, for the reason that at first the meteorites are too sparse for many collisions to occur, and that, finally, the outliers of the major swarm are drawn within the orbit of the smaller one, so that it passes clear. The tables show that this view is entirely consistent with the facts observed, for the greater number of instances of variability occur in the case of those stars in which on other grounds mean spacing seems probable.

*The Cases of Small Range.*

So far, to account for the greatest difference in luminosity at periastron passage, we have supposed the minor swarm to be only involved in the larger one during a part of its revolution, but we can easily conceive a condition of things in which its orbit is so nearly circular that it is almost entirely involved in the larger swarm. Under these conditions, collisions would occur in every part of the orbit, and they would only be more numerous at periastron in the more condensed central part of the swarm, and it is to this that I ascribe the origin of the phenomena in those objects—a very small number—in which the variation of light is very far below the normal range, one or two magnitudes instead of six or seven. Of course, if we imagine two subsidiary swarms, the kind of variability displayed by such objects as $\beta$ Lyræ is easily explained.

*Study of Light Curves.*

I owe to the kindness of Mr. Knott the opportunity of studying several light curves of "stars" of this group, and they seem to entirely justify the explanation which I have put forward. It is necessary, however, that the curves should be somewhat carefully considered because in some cases the period of the minimum is extremely small, as if the secondary body scarcely left the atmosphere of the primary one but was always at work. But when we come to examine the shape of the curves more carefully what we find is that the rise to maximum is extremely rapid; in the case of U Geminorum for instance there is a rise of five magnitudes in a day and a half; whereas the fall to minimum is relatively slow. The possible explanation of this is that the rise of the curve gives us the first sudden luminosity due to the collisions of the swarms, while the descent indicates to us the gradual toning down of the disturbance. If it be considered fair to make the descending curve from the maximum exactly symmetrical with the ascending one on the assumption that the immediate effect produced is absolutely instantaneous, then we find in all cases that I

have so far studied that the star would continue for a considerable time at its minimum.

Broadly speaking, then, we may say that the variables in this group are *close doubles.* The invisibility of the companion being due to the nearness to the primary or to its faintness.

*Double Stars.*

If, in connexion with this subject, we refer to the various observations which have been made of double nebulæ and stars, we are driven to the conclusion that in many cases a double star has at one time existed as a double nebula, while on the other hand, from what has been stated it seems probable that in many cases the companion is a late addition to the system. It would seem as if we may be able in the future, by observing the spectra of double stars, or possibly even their colours when once each particular colour has been attached to a particular spectrum, to discriminate between these two conditions.

In discussing this matter, however, a difficulty arises on account of the fact that on the new view there will be no constant relation between the mass of a swarm and its brightness. When we see a "star" of a certain magnitude, we cannot tell from its brightness alone whether it is a large faint one or a small bright one, for a large body at a low temperature may be equalled or even excelled in brightness by a smaller "star" at a higher temperature. But when we know the spectra of the bodies, we also know their relative temperatures. In the absence of spectroscopic details, colour helps us to a certain extent.

If a pair of "stars" of unequal masses have condensed from the same nebulosity, the smaller one will be further advanced along the temperature curve than the larger one, and the colours and spectra will be different; *but it is not imperative that the magnitudes shall be unequal,* for the smaller swarm will for a time be considerably hotter than the larger one.

If the masses be very unequal, the smaller one will have the smaller magnitude for the longest time. Where there is a great difference in magnitude, therefore, it is generally fair to assume that the one with the smaller magnitude has also the smaller mass.

Another difficulty in the discussion, in the absence of spectroscopic details, is due to the similarity in colour of bodies at opposite points of the temperature curve. Thus, bodies in Group III have, as far as we at present know, exactly the same colour, namely, yellow, as those in Group V. Again, many of the members of Group II have the same colour as some in Group VI.

*The general conditions* with regard to this subject may be thus *briefly stated:—If* the *magnitudes*, colours, and spectra of the two

components of a physical double are identical, both had their origin in the same nebulosity.

If the *magnitudes* are nearly equal, but the colours and spectra different, it may be that the one with the most advanced spectrum has the smaller mass, and if the advance is in due proportion, we are justified in regarding them as having had a common origin.

If the *magnitudes* are very unequal, we may take the one with the smaller magnitude as having the smaller mass, and if it is proportionately in advance, as indicated by its spectrum or colour, we may regard both components as having had a common origin. If the smaller one be less advanced than the larger one, as most generally happens, we have to regard it as a late addition to the system.

If the two stars are of equal *mass* and revolve round their common centre of gravity they have in all probability done so from the nebulous stage, and therefore they will have arrived at the same stage along the evolution road, and their colours and spectra will be identical.

If, however, the *masses* are very different, then the smaller mass will run through its changes at a much greater rate than the larger one. In this way it is possible that the stars seen so frequently associated with globular nebulæ may be explained; while the nebula with a larger mass remains still in the nebulous condition, the smaller one may be advanced to any point, and may indeed even be totally invisible, while the parent nebula is still a nebula. This condition may be stated most generally by pointing to those double stars in which the companions are small and red, although we know nothing for certain with regard to their masses. But if we pass to the other category in which the companion is added afterwards, the most extreme form would be a nebula revolving round a completely formed star; a less extreme form would be a bright line star, or a star of the second group, revolving round it. In this case the colour would be blue or greenish-blue or gray; now this is the greatly preponderating condition, as I have gathered from a discussion of the colours of the small companions given in Smyth's 'Celestial Cycle'; and accepting these colours alone, we should be led to think that most of the small companions of our present stars were not companions originally, but represent later additions to the systems.

It is obvious that there are very many other questions of great interest lying round these considerations, but it is not necessary that I should refer at greater length to them on this occasion, as my present object is only to show that a consideration of the colours of double stars really adds weight to the cause of variability which I have suggested.

[Received April 9, 1888].

CONCLUSION.

Although in this paper I have chiefly confined myself to the discussion of the probable nature of the bodies in Groups I and II, I have also been engaged in the investigation of the spectra of some of the bodies included in the remaining groups, with a view to their detailed classification. Here, however, the work goes on slowly for lack of published material, especially with regard to the examination of the stars which should be included in Groups III and V. With regard to Group VI, however, I may state that all the stars the spectra of which have been recorded have been distributed among five well-marked species, and that there is evidence that some of the absorption is produced by substances which remain in the atmosphere during the next stage, that of Group VII. This probability is based upon the fact that some of the bands are apparently coincident with bands in the telluric spectrum as mapped by Brewster, Ångström, Smyth, and others.

In special connexion with the discussion of Groups I and II, the spectrum of the Aurora Borealis, concerning which I have already (January 19, 1888) communicated to the Society a preliminary note indicating the possible connexion between the spectra of the aurora and of stars of Group II, has been further studied. By this inquiry the work has been advanced a stage, and the view is strengthened that in the case of the aurora the spectrum is mainly one of metallic flutings and lines, probably produced by electric glows in an atmosphere charged with meteoric dust and the *débris* of shooting stars; while in bodies of Groups I and II it is chiefly produced by collisions between the component meteorites.

It may be thought by some premature to give an extended discussion of the bodies belonging to the two groups which have been dealt with before my view of their constitution has been thoroughly tested by observations. My reasons, however, for the present publication are twofold. I have not sufficient optical power at my disposal to go over the ground myself, and I have been anxious to save time by indicating to those who are at present occupied with stellar spectra, or who may be prepared to undertake such observations with sufficient optical appliances, the points chiefly requiring investigation as being of a crucial nature.

From this point of view the small number of observatories paying attention to these matters is much to be regretted, and the importance of Mrs. Draper's noble endowment of spectroscopic photography at Harvard College will be best appreciated.

I may, however, say that I have made some observations in the

clear air of Westgate-on-Sea, with a fine 12-inch mirror which has been kindly lent to me by Mr. Common, which have convinced me of the existence of bright carbon flutings in $\alpha$ Orionis. This is the most crucial observation I have been able to suggest.

The necessity for the employment of large apertures in the investigation is shown by the fact that with Mr. Common's mirror I was totally unable to see any lines in the spectrum of $\gamma$ Cassiopeiæ except the red line of hydrogen.

The laboratory researches on the spectra of meteorites are also being continued. I am glad to be permitted to state that the meteorites employed from the commencement of my work are fragments of undoubted authenticity which have been placed at my disposal by the Trustees of the British Museum, and my best thanks are due to that body.

I have also to thank Professor Flower and Mr. Fletcher, the official in charge of the Mineral Department, for their kindness in giving me special facilities for studying our national collections.

Finally, as before, I have to thank my assistants, Messrs. Fowler, Taylor, and Richards for the manner in which they have helped me throughout these inquiries. Their intelligent and unflagging zeal have rendered me greatly their debtor.

I also wish to thank Mr. Collings for the care with which the illustrations have been prepared.

## *Presents, April* 12, 1888.

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Papers relative to the Royal Society formerly in the possession of Sir Joseph Banks, including several autograph documents. The Hon. Edw. Stanhope, M.P.

*April* 19, 1888.

Admiral Sir G. H. RICHARDS, K.C.B., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks orde for them.

The Right Hon. Lord Sudeley was admitted into the Society.

The following Papers were read:—

I. "The Radio-Micrometer." By C. V. BOYS, A.R.S.M. Co municated by Professor A. W. RÜCKER, F.R.S. Receiv March 8, 1888.

(Abstract.)

In the full paper I have treated the subject of the radio-micromet in such a manner as to arrive at the best proportions of the instr ment. But I have first referred to the fact that the invention of instrument of the kind was originally made by M. d'Arsonval, and was in ignorance of this that I sent in my preliminary note.

The instrument consists essentially of a thermo-electric circ suspended by a torsion fibre in a strong magnetic field. At first have shown that the parts cannot be too thin nor the circuit too sm until the limits imposed by practical considerations make further duction objectionable. I have made the circuit of a bar of antimo and bismuth, with the ends joined by a hoop of copper wire.

I have at first taken the bar as an invariable, and shown how t copper wire may be proportioned to it to give the best results.

By "best" may be meant that which will give the greatest defl tion, either for the weight or for the moment of inertia of the s pended parts.

Calling

$W$ the weight of the bar and mirror (invariable),
$w$ the weight of the copper wire (variable),
$C$ the resistance of the bar (invariable),
$r$ the resistance of the copper wire (variable),
$l$ the length of the rectangle of copper, supposed 1 cm. wid
$u'$ *the weight* of a piece of copper of unit dimensions,
$v$ *the resistance* of a piece of copper of unit dimensions,
$a$ *the sectional* area of the copper wire,

I have shown that—

$$\text{The best sectional area, } a = \sqrt{\left(\frac{\mathrm{W}v}{u'\mathrm{C}}\right)},$$

$$\text{Twice the best length, } 2l = 1 + \sqrt{\left(\frac{\mathrm{CW}}{u'v}\right)},$$

and that the best number of turns of wire is 1.

The numerical values for a particular bar $10 \times 5 \times \frac{1}{4}$ mm. are—

$$a = 0{\cdot}002007 \text{ sq. cm.,} \qquad l = 4{\cdot}621 \text{ cm.}$$

If the breadth be also a variable, the best rectangle is a square of infinite size made of the samo wire, which is always the best, whatever shape, size, or number of turns the circuit may have.

The best circuit with respect to moment of inertia is that which is practically required, because a convenient period of oscillation must be made use of, and so the torsion must be supposed to vary as the moment of inertia. A difficulty was found in working the expression for this, which was entirely overcome by supposing the wire where it crosses the axis to have a sectional area proportional to its distance from the axis, except in its immediate neighbourhood. On this supposition the resistance and the moment of inertia of the upper side of the rectangle are each equal to that of half the same length of copper wire on the sides, and thus not only has the best variation been found, but, what is more important, the coefficients for resistance and moment of inertia have been made identical, which is required in order to put the equations into a simple form.

The expressions found with respect to weight are now applicable to moment of inertia if certain changes are made. Thus, the figure 1 in the expression for length must be replaced by $\frac{1}{2}$. The moment of inertia of the active bar K must replace its weight W, and the moment of inertia of a unit piece of copper at 5 mm. from the axis $u$ must replace its weight $u'$.

It is thus found that the expressions for

$$\text{The best sectional area, } a = \sqrt{\left(\frac{\mathrm{K}v}{u\mathrm{C}}\right)}.$$

And this is true whatever length or number of turns the circuit may have.

$$\text{Twice the best length, } 2l = \tfrac{1}{2} + \sqrt{\left(\frac{\mathrm{KC}}{uv}\right)}.$$

As before, the best number of turns is 1.

The *numerical values are*—

$$a = 0{\cdot}00102 \text{ sq. cm.,} \qquad l = 2{\cdot}337 \text{ cm.}$$

These expressions give the proportions which will produce the greatest deflection. But in case of a strong magnet the resistance to the motion is so great as to be more than sufficient to make the movement dead beat, and this is inconvenient. I have therefore introduced the effect of this resistance into the equations, and found expressions for the best circuit that is just dead beat.

Calling $H'$ the least magnetic field that will make the circuit dead beat,
$G$ the conductivity of the whole circuit,
$K'$ the moment of inertia of the whole circuit,

I have shown that—

$$H' = 2\sqrt{\frac{\pi}{\tau}} \cdot \frac{\sqrt[4]{K'}}{l\sqrt{G}},$$

and that the greatest sensibility of a circuit that is just dead beat is—

$$S = 2\sqrt{\frac{\pi}{\tau}} \cdot \frac{\sqrt{G}}{K'^{\frac{1}{4}}}.$$

From these it is found that the best sectional area is reduced to about three-fourths its previous value, but that the shorter the rectangle of copper the better, until the greatest magnetic field that can be made use of practically is reached.

On considering variations of breadth in the circuit, it is found that if the upper side of the rectangle—that which crosses the axis—is neglected, the sensibility is independent of the breadth, and that the following relations hold:—

$$\text{Best } a = \frac{1}{b}\sqrt{\frac{Kv}{uC}},$$

$$\text{Best } l = \frac{1}{2b}\sqrt{\frac{KC}{uv}},$$

when $b$ is the breadth, and that what I have called the greatest efficacy $E_k$, *i.e.*, sensibility in a given field, is—

$$E_k = \frac{1}{8\sqrt{(KCuv)}}.$$

Since the cross wire becomes increasingly mischievous with an increasing breadth of circuit, $b$ cannot be made too small.

Further, it appears that the copper wire should have the same moment of inertia and resistance as the invariable parts of the circuit.

Other expressions are given, but it may be sufficient to state here

t the circuit which is best according to the rules given by these ations is seven times as good as the best previously found.

have then shown that the mirror must be of such a size as to re a moment of inertia one-third of that of the active bar. In particular case considered, where the active bar consists of two ces, one antimony and one bismuth, $5 \times 1 \times \frac{1}{4}$ mm., at a mean tance of 1 mm. apart, the diameter of the mirror should be $2\frac{3}{4}$ mm. is size both theoretically should, and practically does, enable one th certainty to observe a deflection of $\frac{1}{4}$ mm. on a scale 1 metre tant.

General considerations show that the antimony-bismuth bars cannot ve too small a sectional area, but that the length when already short only involved in a secondary manner.

It is shown that the heat in the circuit is equalised mainly by conction, which is thirty times as effective as the Peltier action.

It is found necessary to screen the antimony and bismuth from the gnetic field by letting them swing in a hole in a piece of soft iron ried in the brass work.

I have shown that the instrument imagined in the preliminary note ould be so much more than dead beat that it would not be possible use it advantageously, but on making a corresponding calculation r the best circuit, now found, using conditions which have been roved by practice to work well together, a difference of temperature one ten-millionth of a degree centigrade is by no means beyond e power of observation.

The figures given by an actual comparison between the newest strument and one of the original pattern is very favourable to the rmer.

In conclusion, I have explained the peculiar action of the rotating ile, and have shown that it is different from that figured in Noad's Electricity and Magnetism.'

## I. "On Hamilton's Numbers. Part II." By J. J. SYLVESTER, D.C.L., F.R.S., Savilian Professor of Geometry in the University of Oxford, and JAMES HAMMOND, M.A., Cantab.

Received March 9, 1888.

(Abstract.)

§ 4. *Continuation, to an infinite number of terms, of the Asymptotic Development for Hypothenusal Numbers.*

In the third section of this paper ('Phil. Trans.,' A., vol. 178, p. 311) *it was stated, on what is now seen to be insufficient evidence, t the asymptotic development of* $p-q$, the half of any hypothenusal

number, could be expressed as a series of powers of $q-r$, the half of its antecedent, in which the indices followed the sequence 2, $\frac{3}{2}$, 1, $\frac{3}{4}$, $\frac{5}{8}$, $\frac{1}{2}$, . . . .

It was there shown that, when quantities of an order of magnitude inferior to that of $(q-r)^{\frac{3}{4}}$ are neglected,

$$p-q = (q-r)^2 + \tfrac{4}{3}(q-r)^{\frac{3}{2}} + \tfrac{11}{18}(q-r) + \tfrac{10}{81}(q-r) \;;$$

but, on attempting to carry this development further, it was found that, though the next term came out $\frac{88}{1215}$ $(q-r)^{\frac{5}{8}}$, there was an infinite series of terms interposed between this one and $(q-r)^{\frac{1}{2}}$.

In the present section it will be proved that between $(q-r)^{\frac{3}{4}}$ and $(q-r)^{\frac{1}{2}}$ there lies an infinite series of terms whose indices are—

$$\tfrac{5}{8},\ \tfrac{9}{16},\ \tfrac{17}{32},\ \tfrac{33}{64},\ \tfrac{65}{128},\ \ldots.$$

and whose coefficients form a geometrical series of which the first term is $\frac{88}{1215}$ and the common ratio $\frac{2}{3}$.

We shall assume the law of the indices (which, it may be remarked, is identical with that given in the introduction to this paper as originally printed in the 'Proceedings') and write—

$$\begin{aligned} p-q = {} & (q-r)^2 + \tfrac{4}{3}(q-r)^{\frac{3}{2}} + \tfrac{11}{18}(q-r) + \tfrac{10}{81}(q-r)^{\frac{3}{4}} \\ & + \frac{2^3}{3^3}A(q-r)^{\frac{5}{8}} + \frac{2^4}{3^4}B(q-r)^{\frac{9}{16}} + \frac{2^5}{3^5}C(q-r)^{\frac{17}{32}} \\ & + \frac{2^6}{3^6}D(q-r)^{\frac{33}{64}} + \frac{2^7}{3^7}E(q-r)^{\frac{65}{128}} + \&c., \textit{ ad inf.} \\ & + \Theta^* \quad \ldots\ldots\ldots\ldots \quad (1.) \end{aligned}$$

The law of the coefficients will then be established by proving that—

$$A = B = C = D = E = \ldots\ldots = \tfrac{11}{45}.$$

If there were any terms of an order superior to that of $(q-r)^{\frac{1}{2}}$, whose indices did not obey the assumed law, any such term would make its presence felt in the course of the work; for, in the process we shall employ, the coefficient of each term has to be determined before that of any subsequent term can be found. It was in this way that the existence of terms between $(q-r)^{\frac{5}{8}}$ and $(q-r)^{\frac{1}{2}}$ was made manifest in the unsuccessful attempt to calculate the coefficient of $(q-r)^{\frac{1}{2}}$.

It thus appears that the assumed law of the indices is the true one.

It will be remembered that $p$, $q$, $r$, . . . . are the halves of the

* *In the text above* $\Theta$ represents some unknown function, the asymptotic value *of whose ratio to* $(q-r)^{\frac{1}{2}}$ is finite.

sharpened Hamiltonian Numbers $E_{n+1}$, $E_n$, $E_{n-1}$, . . . . and that consequently the relation—

$$E_{n+1} = 1+\frac{E_n(E_n-1)}{1\,.\,2}-\frac{E_{n+1}(E_{n-1}-1)(E_{n-1}-2)}{1\,.\,2\,.\,3}+\,.\,.\,.\,.$$

may be written in the form—

$$p = \tfrac{1}{2}+\frac{q(2q-1)}{2}-\frac{r(2r-1)(2r-2)}{2\,.\,3}+\frac{s(2s-1)(2s-2)(2s-3)}{2\,.\,3\,.\,4}$$

$$-\frac{t(2t-1)(2t-2)(2t-3)(2t-4)}{2\,.\,3\,.\,4\,.\,5}$$

$$+\frac{u(2u-1)(2u-2)(2u-3)(2u-4)(2u-5)}{2\,.\,3\,.\,4\,.\,5\,.\,6}$$

$$-\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\quad (2.)$$

The comparison of this value of $p$ with that given by (1) furnishes us with an equation which, after several reductions have been made in which special attention must be paid to the order of the quantities under consideration, ultimately leads to the determination of the values of A, B, C, . . . . in succession.

## III. "Hydraulic Problems on the Cross-sections of Pipes and Channels." By HENRY HENNESSY, F.R.S., Professor of Applied Mathematics and Mechanism in the Royal College of Science for Ireland. Received March 14, 1888.

In that division of hydromechanics which is devoted to the investigation of the flow of liquids through pipes and open channels, the resistance due to the friction of the contained liquid against the sides of the pipes or channels has led to expressions for the velocity as a function of the dimensions and shape of the cross-section commonly designated as the hydraulic mean depth.

This quantity is defined as the quotient of the area of the cross-section of the liquid by that part of its perimeter in contact with the pipe or channel. In a full pipe this perimeter is identical with that of the pipe's cross-section, and in practice this is generally a circle.

It is also proved from the Calculus of Variations that a circle is the closed curve which, under a given perimeter, has the largest area, and by the same processes of analysis a segment of a circle appears to be that which includes the greatest area between its arc and its chord.

If we call the hydraulic *mean depth* of a pipe or channel bounded *by a curved outline u, its definition* gives the condition

$$u = \frac{\int y\,dx}{\int dx\sqrt{\left\{1+\left(\frac{dy}{dx}\right)^2\right\}}},$$

where the limits of the integrals are taken between the same p( on the curve.

If $l = \int dx \sqrt{\left\{1+\left(\frac{dy}{dx}\right)^2\right\}}$ is given, then the problem is to find curve which makes $\int y\,dx$ a maximum for the given value of $l$. Th a well-known isoperimetrical problem*, for by the principles of Calculus of Variations we have in this case—

$$\delta\int\left(y + a\sqrt{1+\left(\frac{dy}{dx}\right)^2}\right)dx = 0,$$

where $a$ is arbitrary, and therefore

$$\frac{1}{a} - \frac{d}{dx}\left(\frac{dy/dx}{\sqrt{\left\{1+\left(\frac{dy}{dx}\right)^2\right\}}}\right) = 0,$$

which gives

$$\frac{x-c}{a} = \frac{dy/dx}{\sqrt{\left\{1+\left(\frac{dy}{dx}\right)^2\right\}}}, \quad a\frac{dy}{dx} = \frac{x-c}{\sqrt{\left\{1-\left(\frac{x-c}{a}\right.\right.}}$$

and $y - c' = \sqrt{\{a^2 - (x-c)^2\}}$ the equation of a circle radius $= a$. This result proves that for a full pipe the circle g the greatest hydraulic mean depth, but it does not tell what is particular arc of a circle which gives the greatest quotient for area of the segment between itself and its chord divided by it This is best done by the ordinary methods of maxima and minin follows :—

Let $\theta$ represent the angle subtended at centre by the segment o: circle whose radius in $r$, then—

$$u = \tfrac{1}{2}r\left(1 - \frac{\sin\theta}{\theta}\right)$$

* In his 'History of the Calculus of Variations,' p. 69, Todhunter has m remark on this problem; namely, that if the curve instead of being closed required to pass through two given fixed points with the arc between these p of a given length, the constants of integration would not be arbitrary, and would be two equations from the fact of the circle passing through the given p *and another arising* from the given length. The solution here given avoid *necessity of two such* equations by employing the well-known properties of *of a circle and its included* segment.—March 29, 1888.

$$\frac{du}{d\theta} = \tfrac{1}{2}r\left(\frac{\sin\theta - \theta\cos\theta}{\theta^2}\right)$$

$$\frac{d^2u}{d\theta^2} = \frac{r}{2\theta^3}\left[(\theta^2-2)\sin\theta + 2\theta\cos\theta\right].$$

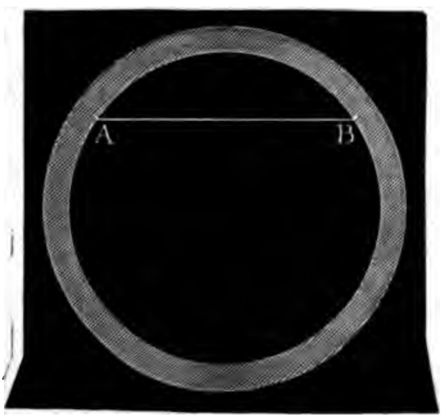
When $$\frac{du}{d\theta} = 0, \qquad \theta = \tan\theta,$$

this may be satisfied either by $\theta = 0$, or by some arc between $\pi$ and $2\pi$. The root $\theta = 0$, substituted in the value of $d^2u/d\theta^2$, makes this positive and equal to $\frac{1}{2}r$, as may be easily shown by expanding $\sin\theta$ and $\cos\theta$. Let now $\theta = \pi + \beta$, and by successive trials we shall find that $\beta = 77°\ 27'$ nearly satisfies the equation $\pi + \beta = \tan\beta$. With this value $\theta$ is $257°\ 27'$, $\cos\theta$ and $\sin\theta$ are both negative and $d^2u/d\theta^2$ is also negative, showing that the result gives a maximum for $u$, which in this case becomes

$$u = \tfrac{1}{2}\,r\,(1 + 0{\cdot}21722) = 0{\cdot}6086\,r, \text{ nearly.}$$

The hydraulic mean depth of a full pipe or of a half-full pipe of circular section is $0{\cdot}5\,r$, hence that for a section less by about three-tenths of the perimeter of the circle is greater. The area of the section of greatest hydraulic mean depth is $2{\cdot}74142\,r^2$ or $0{\cdot}87169$ of the entire circle. If the pipe is nearly horizontal the quantity of liquid contained in it is proportional to the cross-section, hence a circular pipe under such condition has the greatest hydraulic mean depth when it is nearly seven-eighths full; or when the liquid has fallen from the full state so as to have its free surface AB the chord of an arc of $102°\ 33'$. The versed sine of this arc is $0{\cdot}1872$ D nearly, D being the

FIG. 1.



diameter, so that for a pipe of 2 feet internal diameter the great hydraulic mean depth would be when the surface of the liquid h fallen below the top by 4·4928, or nearly $4\frac{1}{2}$ inches. As the veloci of the liquid is nearly as the square root of the hydraulic mean dept the pipe filled to this height would carry liquid with a veloci slightly greater than when completely full. This conclusion is on true when the effective head of liquid is due solely to the inclination the pipe. When the level of the liquid within the pipe falls t hydraulic mean depth tends towards its minimum value, and decrease becomes rapid as the arc diminishes; thus if $\theta$ is a very sm angle

$$u = \tfrac{1}{2} r \left(1 - \frac{\sin\theta}{\theta}\right) = \frac{r\theta^2}{12}\left(1 - \frac{\theta^2}{20}\right).$$

But $r = \mathrm{L}/\pi$, where L is the length of an arc of a semicircl hence if the 4th power of $\theta$ is negligible we have $u = \mathrm{L}\theta^2/12\pi$.

Although pipes and conduits for water supply are usually quite fu those for drainage purposes are most commonly only partly filled wi liquid, and the amount of liquid is liable to great fluctuations. Tl has led to the adoption for drainage pipes of an oval curve for t outline of cross-section, with the longer axis of the oval vertical a terminated at bottom by an arc of greater curvature than at to The form of this cross-section suggests an inquiry as to how far curve which has been often treated in isoperimetrical problems wou satisfy the conditions for giving a favourable hydraulic mean dep in an open channel with fluctuating contents. We have seen that particular arc of a circle gives a maximum for the quotient of t area of the segment divided by the perimeter of the arc, and we sh find that there is a particular catenary which gives a maximum foı corresponding quotient of the area included between its perimeter a its chord.

If as usual we make the directrix the axis of $x$, $a$ the paramet and $l$ the length of the curve, then adopting the usual notation

$$x = a \log\left(\frac{y + \sqrt{(y^2 - a^2)}}{a}\right), \qquad \text{and } y = \sqrt{(l^2 + a^2)},$$

but in this case, as the area whose quotient divided by the perimet is to be a maximum is the difference between the rectangle under t coordinates $x$ and $y$ and the area included between the curve, parameter, and the directrix, we have manifestly—

$$u = \frac{xy - \int y dx}{l},$$

and as $\int y dx = al$, this may be written

[redacted]



$$u = \frac{a\sqrt{(l^2 + a^2)}\log\left(\frac{l + \sqrt{(l^2 + a^2)}}{a}\right) - al}{l}.$$

The shape of the curve depends on the relation between its parameter and its length, hence we must find the value of $a$ which makes $u$ a maximum in the above expression. The problem seems therefore to amount to this elementary statical question:—A flexible and uniform chain is attached to two supports on the same horizontal line; required the distance between the supports so as to make the area of the surface included between the chain and the horizontal line the greatest possible; or given the perimeter of a catenary to find the chord, so that the area between itself and the curve shall be a maximum. The above expression gives—

$$\begin{aligned} l\frac{du}{da} &= \sqrt{(l^2 + a^2)}\log\left(\frac{l + \sqrt{(l^2 + a^2)}}{a}\right) \\ &\qquad + \frac{a^2}{\sqrt{(l^2 + a^2)}}\log\left(\frac{l + \sqrt{(l^2 + a^2)}}{a}\right) \\ &\qquad + a\sqrt{(l^2 + a^2)}\frac{d}{da}\left[\log\left(\frac{l + \sqrt{(l^2 + a^2)}}{a}\right)\right] - l \\ &= \frac{l^2 + 2a^2}{\sqrt{(l^2 + a^2)}}\log\left(\frac{l + \sqrt{(l^2 + a^2)}}{a}\right) - 2l. \end{aligned}$$

$$l\frac{d^2u}{da^2} = \frac{(3l^2 + 2a^2)\,a^2\log\left(\frac{l + \sqrt{(l^2 + a^2)}}{a}\right) - l\,(l^2 + 2a^2)\sqrt{(l^2 + a^2)}}{a\,(l^2 + a^2)^{3/2}}.$$

If we write $z = l/a$, and make $du/da = 0$, we have

$$\log(z + \sqrt{(1 + z^2)}) = \frac{2z\sqrt{(1+z^2)}}{2 + z^2}.$$

By successive trials this equation may be satisfied by substituting $z = 2{\cdot}4$, whence $l = 2{\cdot}4a$. This value substituted in the expression for $d^2u/da^2$ gives a negative result, and therefore the value of $u$ is a maximum when $a = \tfrac{5}{12}l$. When $z = 2{\cdot}4$, $\sqrt{(1 + z^2)} = 2{\cdot}6$, and $\log(z + \sqrt{(1 + z^2)}) = \log 5 = 1{\cdot}6094$ nearly.

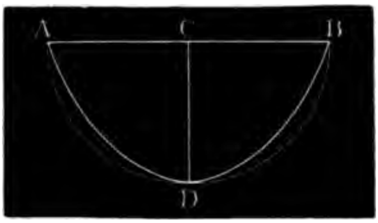
$$\frac{2z\sqrt{(1+z^2)}}{2 + z^2} = \frac{4{\cdot}8\,.\,2{\cdot}6}{7{\cdot}76} = 1{\cdot}6082.$$

With further approximation we should find therefore—

$$x = \frac{l}{2{\cdot}4}\log 5 = \frac{2}{3}l \text{ nearly}, \qquad y = \frac{2{\cdot}6}{2{\cdot}4}l = \frac{13}{12}l.$$

But the depth $h$ of the curve below its chord is $y - a$ or $h$
In this inquiry $l$ is the perimeter of the half curve, so that th
perimeter, the chord, and the depth are respectively in the r
the numbers 3, 2, and 1, or the chord of the catenary of greate
for a given perimeter is twice the depth, and the length of the
is three times the depth. The outline of this curve is readily
by attaching a fine chain of 3 units of length to supports at a d

FIG. 2.




The chord AB = 2CD.
The arc ADB of the catenary = 3CD.

of 2 units. The catenary which would give a maximum hy
mean depth for an open channel is therefore one whose depth
radius and chord the diameter of a semicircle. On substituti
value of $a$ found above in the equation for $u$, we shall find th
hydraulic mean depth of the catenary under consideration is
$0{\cdot}31l$ or $0{\cdot}155\mathrm{L}$, where L is the total perimeter of the curve.
semicircle the hydraulic mean depth is $\frac{1}{2}r = \mathrm{L}/2\pi$, or
nearly, hence the hydraulic mean depth of the catenary of ma
area is nearly equal to that of a semicircle of equal perimeter
a channel formed by the outline of such a catenary would wh
contained liquid falls, not be liable to so rapid a reduction of hy
mean depth as in the semicircle. For small arcs of a circle
been shown that this is proportional to the square of the ang
tended at the centre. In the catenary if $\theta$ is the angle made
tangent with the directrix, it is also the angle made by the
of curvature with the axis of $y$, which in this case coincides w
axis of depth, and as

$$y = a \sec\theta, \quad x = a \log(\sec\theta + \tan\theta), \quad l = a \tan$$

$$u = \frac{a}{\tan\theta}\left[\sec\theta \log(\sec\theta + \tan\theta) - \tan\theta\right]$$

$$= \frac{a}{\sin\theta}\left[\log \tan \tfrac{1}{2}(\pi + \theta) - \sin\theta\right]$$

$$= a\,[\operatorname{cosec}\theta\,.\,\log\tan\tfrac{1}{2}(\pi+\theta)-1]$$

$$= a\,[2\operatorname{cosec}\theta\,(\tan\tfrac{1}{2}\theta+\tfrac{1}{3}\tan^3\tfrac{1}{2}\theta+\&c.-1)$$

$$= a\left[\left(\frac{2}{\theta}+\frac{\theta}{3}+\quad\right)\left(\tfrac{1}{2}\theta+\frac{\theta^3}{24}\right)-1\right]$$

$$= \frac{a\theta^2}{4},$$

when $\theta^3$ and higher powers are omitted; and remembering that $a = l/(2{\cdot}4) = L/(4{\cdot}8)$, we may write for such an arc—

$$u = L\theta^2/(19{\cdot}2).$$

An arc of the semicircle at its base subtending the angle $\theta$ has when $\theta$ is small the value $L\theta^2/12\pi$, as already pointed out. Hence for a circular channel and for one formed by a catenary of equal perimeter and maximum area, the hydraulic mean depth for small segments subtending equal angles would be greater for the latter. On looking at the outline of such a catenary inscribed in a semicircle, this result seems to be confirmed, and the curve approaches the oval which experience has led engineers to adopt for the section of pipes carrying fluctuating quantities of liquid.

The general result of the preceding inquiry may be summed up in the following conclusions:—For all pipes and conduits employed to convey liquid for consumption or for milling power, the circular section is the best, as the level of the liquid in the pipe is rarely, if ever, below half the diameter.

For drainage such a form is also the best if the liquid rarely falls below half the diameter, but if it is liable to fall nearly to the bottom of the pipe or conduit, an oval form, such as that actually recommended, is the best. If the pipe is likely to be as often half full as slightly filled, it is probable that some advantage would be gained by employing the catenary of maximum area for a given perimeter for the lower part of the oval. A pattern for this form can be always readily constructed by remembering the relations 1, 2, 3 for the depth, the chord, and the length of the curve. In designing the base of the pipe, it is only necessary, as already pointed out, to hang a fine chain of 3 units between supports placed at 2 units on the same horizontal line.

It is well known that in a triangular notch or triangular channel, the sides of which are at right angles, the velocity of the liquid varies but little with the depth, and it is possible to conceive that a channel may have such a form as to make such a variation extremely small.

If we suppose the surface of the liquid in an open channel to be bounded by the chord of the cross-section of the channel, then we shall have as before the hydraulic mean depth—

$$u = \frac{xy - \int y dx}{\int dx \sqrt{\left(1 + \left(\frac{dy}{dx}\right)^2\right)}},$$

and if we make $u =$ constant—

$$x\frac{dy}{dx}\int dx \sqrt{\left\{1 + \left(\frac{dy}{dx}\right)^2\right\}} = (xy - \int y dx)\sqrt{\left\{1 + \left(\frac{dy}{dx}\right)^2\right\}},$$

the limits of the integrals in both cases being taken on the same points of the curve.

From this it follows that—

$$x\frac{dy}{dx} = c\sqrt{\left\{1 + \left(\frac{dy}{dx}\right)^2\right\}},$$

which on integrating gives

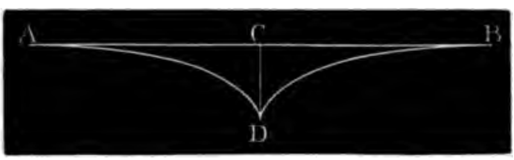
$$y = c \log \{x + \sqrt{(x^2 - c^2)}\} + C.$$

This result indicates a catenary with its convexity turned to the chord and to the axis of $y$, but between the limits $x = 0$ and $x = x$ the value of $y$ becomes imaginary, the constant $c$ being the hydraulic mean depth, which must be very small in such a case as here supposed, if we take $x$ from $x = c$ to $x = x$

$$y = c \log \left(\frac{x + \sqrt{(x^2 - c^2)}}{c}\right),$$

and such a notch or channel might be approximately realised by two arcs of a catenary with parameters corresponding to the small arbitrary value of $c$.

FIG. 3.




A notch or channel with such a cross-section would have an almost constant hydraulic mean depth, but it would be inapplicable to any useful purposes in the application of hydraulics.

*The cross-sections* of rivers and navigable canals are regarded *chiefly with reference* to permanence, and the question of their *hydraulic mean depth* is less important than in the case of water

supply and drainage pipes. In canals the trapezoidal section is that which experience has almost universally established as the best wherever canals are carried through ordinary earth, and the rectangular section is only adopted when the sides are composed of coherent matter such as rock or masonry. The semicircular section for an open channel would not approximate to the shapes usually adopted in canals, but it may be worth remarking that the outline of the catenary of greatest area approaches more nearly to such shapes.

## IV. "On the Heating Effects of Electric Currents. No. III." By W. H. Preece, F.R.S. Received March 15, 1888.

I have taken a great deal of pains to verify the dimensions of the currents, as detailed in my paper read on December 22, 1887, required to fuse different wires of such thicknesses that the law

$$C = ad^{3/2}$$

is strictly followed; and I submit the following as the final values of the constant "$a$" for the different metals:—

| | Inches. | Centimetres. | Millimetres. |
|---|---|---|---|
| Copper.................. | 10,244 | 2,530 | 80·0 |
| Aluminium.............. | 7,585 | 1,873 | 59·2 |
| Platinum................ | 5,172 | 1,277 | 40·4 |
| German silver............ | 5,230 | 1,292 | 40·8 |
| Platinoid................ | 4,750 | 1,173 | 37·1 |
| Iron.................... | 3,148 | 777·4 | 24·6 |
| Tin .................... | 1,642 | 405·5 | 12·8 |
| Alloy (lead and tin 2 to 1) | 1,318 | 325·5 | 10·3 |
| Lead.................... | 1,379 | 340·6 | 10·8 |

With these constants I have calculated the two following tables, which I hope will be found of some use and value:—

Table showing the Current in Ampères required to Fuse Wires of Various Sizes and Materials.

$$C = ad^{3/2}.$$

| No. S.W.G. | Diameter. Inches. | $d^{3/2}$. | Copper. $a = 10{,}244$. | Aluminium. $a = 7585$. | Platinum. $a = 5172$. | Ger. Silver. $a = 5230$. | Platinoid. $a = 4750$. | Iron. $a = 3148$. | Tin. $a = 1642$. | Tin-lead alloy. $a = 1318$. | Lead. $a = 1379$. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 14 | 0·080 | 0·022627 | 231·8 | 171·6 | 117·0 | 118·3 | 107·5 | 71·22 | 37·15 | 29·82 | 31·20 |
| 16 | 0·064 | 0·016191 | 165·8 | 122·8 | 83·73 | 84·68 | 76·90 | 50·96 | 26·58 | 21·34 | 22·32 |
| 18 | 0·048 | 0·010516 | 107·7 | 79·75 | 54·37 | 54·99 | 49·95 | 33·10 | 17·27 | 13·86 | 14·50 |
| 20 | 0·036 | 0·006831 | 69·97 | 51·81 | 35·33 | 35·72 | 32·44 | 21·50 | 11·22 | 9·002 | 9·419 |
| 22 | 0·028 | 0·004685 | 48·00 | 35·53 | 24·23 | 24·50 | 22·25 | 14·75 | 7·692 | 6·175 | 6·461 |
| 24 | 0·022 | 0·003263 | 33·43 | 24·75 | 16·88 | 17·06 | 15·50 | 10·27 | 5·357 | 4·300 | 4·499 |
| 26 | 0·018 | 0·002415 | 24·74 | 18·32 | 12·49 | 12·63 | 11·47 | 7·602 | 3·965 | 3·183 | 3·330 |
| 28 | 0·0148 | 0·001801 | 18·44 | 13·66 | 9·311 | 9·416 | 8·552 | 5·667 | 2·956 | 2·373 | 2·483 |
| 30 | 0·0124 | 0·001381 | 14·15 | 10·47 | 7·142 | 7·222 | 6·559 | 4·347 | 2·267 | 1·820 | 1·904 |
| 32 | 0·0108 | 0·001122 | 11·50 | 8·512 | 5·805 | 5·870 | 5·330 | 3·533 | 1·843 | 1·479 | 1·548 |

$$d = \left(\frac{C}{a}\right)^{2/3}.$$

| Current in ampères. | Diameter in inches. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Copper. $a$ = 10,244. | Aluminium. $a$ = 7585. | Platinum. $a$ = 5172. | Ger. Silver. $a$ = 5230. | Platinoid. $a$ = 4750. | Iron. $a$ = 3148. | Tin. $a$ = 1642. | Tin-lead alloy. $a$ = 1318. | Lead. $a$ = 1379. |
| 1 | 0·0021 | 0·0026 | 0·0033 | 0·0033 | 0·0035 | 0·0047 | 0·0072 | 0·0083 | 0·0081 |
| 2 | 0·0034 | 0·0041 | 0·0053 | 0·0053 | 0·0056 | 0·0074 | 0·0113 | 0·0132 | 0·0128 |
| 3 | 0·0044 | 0·0054 | 0·0070 | 0·0069 | 0·0074 | 0·0097 | 0·0149 | 0·0173 | 0·0168 |
| 4 | 0·0053 | 0·0065 | 0·0084 | 0·0084 | 0·0089 | 0·0117 | 0·0181 | 0·0210 | 0·0203 |
| 5 | 0·0062 | 0·0076 | 0·0098 | 0·0097 | 0·0104 | 0·0136 | 0·0210 | 0·0243 | 0·0236 |
| 10 | 0·0098 | 0·0120 | 0·0155 | 0·0154 | 0·0164 | 0·0216 | 0·0334 | 0·0386 | 0·0375 |
| 15 | 0·0129 | 0·0158 | 0·0203 | 0·0202 | 0·0215 | 0·0283 | 0·0437 | 0·0506 | 0·0491 |
| 20 | 0·0156 | 0·0191 | 0·0246 | 0·0245 | 0·0261 | 0·0343 | 0·0529 | 0·0613 | 0·0595 |
| 25 | 0·0181 | 0·0222 | 0·0286 | 0·0284 | 0·0303 | 0·0398 | 0·0614 | 0·0711 | 0·0690 |
| 30 | 0·0205 | 0·0250 | 0·0323 | 0·0320 | 0·0342 | 0·0450 | 0·0694 | 0·0803 | 0·0779 |
| 35 | 0·0227 | 0·0277 | 0·0358 | 0·0356 | 0·0379 | 0·0498 | 0·0769 | 0·0890 | 0·0864 |
| 40 | 0·0248 | 0·0303 | 0·0391 | 0·0388 | 0·0414 | 0·0545 | 0·0840 | 0·0973 | 0·0944 |
| 45 | 0·0268 | 0·0328 | 0·0423 | 0·0420 | 0·0448 | 0·0589 | 0·0909 | 0·1052 | 0·1021 |
| 50 | 0·0288 | 0·0352 | 0·0454 | 0·0450 | 0·0480 | 0·0632 | 0·0975 | 0·1129 | 0·1095 |
| 60 | 0·0325 | 0·0397 | 0·0513 | 0·0509 | 0·0542 | 0·0714 | 0·1101 | 0·1275 | 0·1237 |
| 70 | 0·0360 | 0·0440 | 0·0568 | 0·0564 | 0·0601 | 0·0791 | 0·1220 | 0·1413 | 0·1371 |
| 80 | 0·0394 | 0·0481 | 0·0621 | 0·0616 | 0·0657 | 0·0864 | 0·1334 | 0·1544 | 0·1499 |
| 90 | 0·0426 | 0·0520 | 0·0672 | 0·0667 | 0·0711 | 0·0935 | 0·1443 | 0·1671 | 0·1621 |
| 100 | 0·0457 | 0·0558 | 0·0720 | 0·0715 | 0·0762 | 0·1003 | 0·1548 | 0·1792 | 0·1739 |
| 120 | 0·0516 | 0·0630 | 0·0814 | 0·0808 | 0·0861 | 0·1133 | 0·1748 | 0·2024 | 0·1964 |
| 140 | 0·0572 | 0·0698 | 0·0902 | 0·0895 | 0·0954 | 0·1255 | 0·1937 | 0·2243 | 0·2176 |
| 160 | 0·0625 | 0·0763 | 0·0986 | 0·0978 | 0·1043 | 0·1372 | 0·2118 | 0·2452 | 0·2379 |
| 180 | 0·0676 | 0·0826 | 0·1066 | 0·1058 | 0·1128 | 0·1484 | 0·2291 | 0·2652 | 0·2573 |
| 200 | 0·0725 | 0·0886 | 0·1144 | 0·1135 | 0·1210 | 0·1592 | 0·2457 | 0·2845 | 0·2760 |
| 225 | 0·0784 | 0·0958 | 0·1237 | 0·1228 | 0·1309 | 0·1722 | 0·2658 | 0·3077 | 0·2986 |
| 250 | 0·0841 | 0·1028 | 0·1327 | 0·1317 | 0·1404 | 0·1848 | 0·2851 | 0·3301 | 0·3203 |
| 275 | 0·0897 | 0·1095 | 0·1414 | 0·1404 | 0·1497 | 0·1969 | 0·3038 | 0·3518 | 0·3413 |
| 300 | 0·0950 | 0·1161 | 0·1498 | 0·1487 | 0·1586 | 0·2086 | 0·3220 | 0·3728 | 0·3617 |

## V. "On the Compounds of Ammonia with Selenium Dioxide." By Sir Charles A. Cameron, V.P.I.C., F.R.C.S.I., and John Macallan, F.I.C. Communicated by Professor Dewar, F.R.S. Received March 19, 1888.

The following experiments were undertaken with the object of determining the action of ammonia upon selenium dioxide. They have resulted in the discovery of two new compounds, which, from what has been ascertained regarding their constitution, may, perhaps, be best designated by the term *selenosamates* or ammonium salts of an acid—selenosamic—yet to be isolated.

*Preparation of Neutral Ammonium Selenosamate.*

Ammonia, which had been carefully dried by passing through a series of potash tubes, was led into a solution of selenium dioxide in absolute alcohol. After being absorbed for some time, minute crystals commenced to deposit, and when complete precipitation had taken place, the liquid portion was filtered off, the crystals washed with alcohol, and dried over sulphuric acid in a vacuum.

The compound formed as above described is a deliquescent salt, which separates from its solution in alcoholic ammonia in minute, but very well-defined hexagonal prisms and pyramids—both forms often occurring in combination. It is a very unstable substance, continuously liberating ammonia, and tending to the formation of a more stable acid salt. Some of the crystals which had been placed in a large stoppered bottle were found after some weeks to be entirely converted into large crystals of the acid salt. It also loses ammonia when treated with alcohol or water; and when its aqueous solution is evaporated in a vacuum, crystals of the acid salt remain. When heated, it is at once converted into the acid salt. On account of its instability, it is best prepared in a partial vacuum, and when dried placed in a stoppered bottle, which should be quite full and kept in a cool place. In this way it may be preserved of definite composition for a considerable time. It is with difficulty, and only partially, converted into ammonium selenite by the action of water upon it. When barium chloride is added to its neutral aqueous solution, only a faint cloudiness is produced, until it is heated, when a slight precipitate forms, but even after standing for weeks and long-continued boiling, only a portion of the selenium precipitates. Addition of excess of ammonia to the solution, however, precipitates a basic barium salt. It is but sparingly soluble in cold alcoholic ammonia. 1·6658 gram of solution from which crystals had deposited, left a residue of 0·0134 gram, reduced to acid salt, which is equivalent to a solubility

of one part in 116 at 12°. Heated with the alcoholic ammonia it lissolves freely, but on cooling, the solution remains long superaturated, crystals continuing to deposit for several days. It is very lightly volatile at ordinary temperatures, both in a vacuum and in a urrent of air. As might be expected, potash at once liberates mmonia from it. Sulphurous acid and stannous chloride reduce it rith separation of selenium. It is only slightly affected by hydrohloric or nitric acid in the cold, but strong sulphuric acid reacts iolently upon it, a portion of the salt being sublimed by the heat volved. Chlorine passed through its aqueous solution converts it ompletely into ammonium selenate,—a reaction which was taken dvantage of for its analysis. 0·7820 gram was dissolved in water, aturated with chlorine, and barium chloride added. The resulting arium selenate weighed 1·5150 gram, equivalent to a percentage of 76·84 of selenium dioxide. The ammonia was estimated by Kjellahl's process, slightly modified on account of the volatility of the substance. 0·5651 gram was mixed roughly with potassium permanganate in a small strong flask by means of a glass rod, after which a thin tube containing 10 c.c. of sulphuric acid mixture was lowered into it, and broken by shaking the flask after it had been well secured with an india-rubber cork. It was then heated to 150° for one hour in a paraffin bath. The contents of the flask distilled with potash yielded 0·13175 gram of ammonia, equivalent to a percentage of 23·32. The results obtained agree with the composition—

$$2NH_3,SeO_2 = NH_4,SeO_2(NH_2).$$

| | Calculated. | Found. |
|---|---|---|
| $SeO_2$ | 76·53 | 76·84 |
| $NH_3$ | 23·47 | 23·32 |
| | 100·00 | 100·16 |

The original alcoholic solution from which the crystals had deposited, was found to contain selenium. In order to ascertain in what form it existed, a portion of the solution was evaporated to dryness n a vacuum. The residue weighing 0·666 gram, treated as before, ielded 1·285 gram of barium selenate, equivalent to 76·53 per cent. f selenium dioxide, the theoretical amount in the above compound, howing that a portion remained in solution after the crystals had eposited. It was considered a matter of interest to ascertain how uch of the nitrogen in this salt would be precipitated by platinum hloride. 0·5772 gram was accordingly taken, platinum chloride oured upon it, alcohol added, and the mixture allowed to stand in he cold. The double chloride obtained weighed 1·5502 gram, equivant to a percentage of 20·59 of ammonia. A second estimation in

which 0·4153 gram was taken, yielded 1·1206 gram of the double chloride, equivalent to 20·69 per cent. of ammonia. In a third estimation, 0·3835 gram was evaporated down with platinum chloride, but the double chloride obtained, 1·0237 gram, showed a rather smaller percentage of ammonia, namely, 20·52. The mean of the first two results, 20·64, is equal to 87·94 per cent. of the total ammonia, and indicates that in addition to the basic nitrogen, about three-fourths of the nitrogen contained in the radical of the salt is precipitated by platinum chloride.

*Preparation of Acid Ammonium Selenosamate.*

A solution of the neutral salt in absolute alcohol was boiled down on the water-bath until crystals were deposited. The liquid portion was then drained off, and the crystals washed with alcohol, and dried in a vacuum. On examination, they proved to consist of an acid salt. It was also found that exposure of the neutral salt in a vacuum over sulphuric acid for thirty hours was sufficient to convert it into the same acid compound. A portion of the salt obtained in the latter way was submitted to analysis. For estimation of the selenium, 0·2 gram was dissolved in water, saturated with chlorine, and precipitated with barium chloride. The resulting barium selenate weighed 0·409 gram, equivalent to a percentage of 81·11 of selenium dioxide. In a second estimation, 0·4268 gram yielded 0·8761 gram of barium selenate, equivalent to 81·42 per cent. of selenium dioxide. Kjeldahl's process was found to be unsuitable in this case for estimating the ammonia, the amount yielded by it being much too low, although a very high temperature was maintained for a considerable time. Combustion with soda-lime also gave insufficient results, owing to a portion of the substance being decomposed by the heat employed, with evolution of nitrogen. Somewhat better results were obtained by Dumas' process: 0·4035 gram yielded 49·9 c.c. of nitrogen at 12° and 771·6 mm., equivalent to a percentage of 18·09 of ammonia. An estimation of the selenium in the dried crystals was also made: 0·1523 gram yielded 0·3134 gram of barium selenate, equivalent to 81·62 per cent. of selenium dioxide. The results thus obtained agree with the formula $3NH_3,2SeO_2 = NH_4,H,(SeO_2NH_2)_2$.

| | Prepared in vacuum. 1. | 2. | Crystallised from alcohol. 3. | Calculated. 4. |
|---|---|---|---|---|
| $SeO_2$ | 81·42 | 81·11 | 81·62 | 81·30 |
| $NH_3$ | 18·09 | — | — | 18·70 |

The salt thus obtained is deliquescent, and easily soluble in alcohol, from which it separates in large prisms. 2·0270 grams of saturated solution left a residue weighing 0·1191 gram, showing a solubility of

one part in sixteen of absolute alcohol at 14°. No hydrate was obtained by evaporating its aqueous solution, but the same crystalline forms were deposited as from alcohol. With barium chloride it behaves similarly to the neutral salt, a partial precipitation taking place only with difficulty. It possesses much greater stability than the neutral salt, but like the latter it is reduced by sulphurous acid and stannous chloride, and oxidised by chlorine. Potash decomposes it with evolution of ammonia, but hydrochloric, nitric, or sulphuric acid has only a slight action upon it in the cold. Kept in a vacuum or in a current of air, it is appreciably volatile at ordinary temperatures. When heated strongly, a portion of it sublimes unchanged, part of it is converted into ammonium selenite, while the remainder is decomposed into ammonia, water, nitrogen, and a residue of fused selenium. In order to estimate the amount of ammonia precipitated by platinum chloride, 0·3140 gram was taken, which yielded 0·6234 gram of the double chloride, equivalent to 15·26 per cent. of ammonia, the amount thus precipitated being equal to 81·60 per cent. of the total amount of ammonia in the salt.

### *Relation of the Selenosamates to Sulphur Compounds.*

It is stated that a compound is formed by the action of ammonia on sulphur dioxide, but the description of its properties shows that it does not correspond with the selenosamates. The latter bodies correspond more closely with the compounds which sulphur trioxide forms with ammonia. The molecule, $SeO_2$, therefore, in these reactions acts similarly to $SO_3$, rather than to what is usually regarded as its sulphur analogue, namely $SO_2$.

In conclusion, we are engaged at present in the production of other selenosamates, and hope to give an account of them at an early date.

## VI. "On the Logarithmic Law of Atomic Weights." By G. JOHNSTONE STONEY, M.A., D.Sc., F.R.S. Received April 16, 1888.

(Abstract.)

This memoir is divided into five sections.

Section 1.—When Newlands pointed out the dependence of the atomicity and other properties of some of the chemical elements upon the order in which their atomic weights succeed one another, and especially when this law was extended by Mendelejeff to all the elements, it became manifest that there exists a mathematical relation between a series of numbers and the successive atomic weights of the elements.

In the first section the reason is pointed out why the search for this law has been fruitless, at least as hitherto pursued by the author. The method he adopted was to plot down the atomic weights as ordinates of a diagram of which the abscissas represented some simple numerical series, and to endeavour to extract information from the resulting curves. In this method atomic weights are represented by lines, the ordinates of the figure. Now in the next section it will appear that, in that case, the curve is represented by the equation—

$$y = k \,.\, [\log (qx)]^3,$$

and is further complicated by $x$ not representing simple integer numbers, but a circular function of them. The search, therefore, by this method was from the first hopeless, as the resulting curve is one which has not been studied by geometers, and of which accordingly the inquirer could not recognise the appearance when presented to him.

In Section 2 another method is pursued. The successive atomic weights, instead of being represented by lines, are represented by volumes. A succession of spheres are taken whose volumes are proportional to the atomic weights, and which may be called *the atomic spheres.* When the radii of these spheres are plotted down on a diagram as ordinates, and a series of integers as abscissas, the general form of the logarithmic curve

$$y = k \log (qx)$$

becomes apparent: and close scrutiny has shown that it expresses the real law of nature. It is the central curve that threads its way through the positions given by observation, and the deviations from it of the positions assigned by the actual atomic weights will be included by making $x$ a circular function of integer numbers, instead of those numbers themselves. The first three terms of this function have been determined.

The issue of the investigation is to show that when such a diagram is formed with ordinates which are the cube roots of the atomic weights referred to hydrogen as unit, so that the ordinates may be the radii of spheres whose volumes represent the atomic weights—

1. The logarithmic curve—

$$y_m = k \,.\, \log (m\alpha),$$

(where $\log k = 0{\cdot}785$

and $\log \alpha = \bar{1}{\cdot}986$)

*threads its way through* the positions plotted down from the observations.

2. In the case of the perissads (the elements of uneven atomicity) the complete curve which includes their perturbations from the central curve is—

$$y_m = k \log \left[ a\left(m + \tfrac{1}{3}\sin\frac{m\pi}{27} + \tfrac{1}{3}\sin\frac{m\pi}{18} + \text{subsequent terms}\right)\right],$$

the next term being probably either—

$$-\tfrac{1}{2}\sin\frac{m\pi}{9}, \qquad \text{or } -\tfrac{2}{3}\sin\frac{m\pi}{9}.$$

3. The form of the function representing the perturbations of the artiads is different, at all events after the third term.

Section 3.—There are other neighbouring logarithmic curves which pursue a course close to the observed positions, and in Section 3 the method adopted in dealing with these curves is described, and the grounds on which they have been successively excluded are stated. The evidence relied on has been, for the most part, that the perturbations from them are less reducible to order.

In Section 4 the curve finally selected is thrown into a polar form, and furnishes a diagram of singular convenience for laboratory use. It presents conspicuously the information which a Newlands and Mendelejeff's table is capable of supplying, with the further advantage of also placing before the eye an intelligible representation of the atomic weights.

The last section contains some observations suggested by the investigation.

## *Presents, April* 19, 1888.

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*April* 26, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Coagulation of the Blood." Preliminary Communication. By W. D. HALLIBURTON, M.D., B.Sc., Assistant Professor of Physiology, University College, London. Communicated by Professor E. A. SCHÄFER, F.R.S. (From the Physiological Laboratory, University College, London.) Received March 20, 1888.

[Publication deferred.]

II. "On the Development of the Electric Organ of *Raia batis*." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by J. BURDON SANDERSON, F.R.S. Received March 21, 1888.

(Abstract.)

The paper consists of a short description of the electric organs found in the skate genus, and of an account of the development of the electric organ of the common grey skate (*Raia batis*).

It is shown that while in some skates (*e.g.*, *Raia batis* and others) the organ is made up of disk-shaped bodies, in others (*e.g.*, *Raia fullonica*) it consists of numerous cup-shaped structures provided with long or short stems.

The disks (with the development of which the paper chiefly deals) consist essentially of three layers, viz., (1) an electric plate in front in which the nerves end; (2) a striated layer which supports the electric plate; and (3) an alveolar layer, posterior to which is a thick cushion of gelatinous tissue. Each disk is formed in connexion with a muscular fibre. In young embryos there is no indication of an electric organ, but in an embryo 6 or 7 cm. in length, some of the muscular fibres at each side of the notochord are found in process of conversion into long slender clubs having their heads nearest the root of the tail.

The club-stage having been reached, the muscular fibre next

assumes the form of a mace, and later the anterior end further expands to form a relatively large disk, while the remainder of the original fibre persists as a slender ribbon-shaped appendage. As the head of the club enlarges to form a disk, it passes through an indistinct cup stage, which somewhat resembles the cups of the adult *Raia fullonica*, hence it may be inferred that in *Raia fullonica* the organ has been arrested in its development. The conversion of the muscular fibre into a club is largely caused by the increase at its anterior end of muscle corpuscles. These corpuscles eventually arrange themselves, either in front of the head of the club, to give rise to the electric plate, or they migrate backwards to form at the junction of the head of the club with its stem the alveolar layer. The striated layer, which is from the first devoid of nuclei, seems to be derived from the anterior striated portion of the club.

The gelatinous tissue between the disks and the connective tissue investing them, are derived from the embryonic connective tissue developing disks.

## III. "On the Occurrence of Aluminium in Certain Vascular Cryptogams." By A. H. Church, M.A., F.C.S. Communicated by Dr. J. H. Gilbert, F.R.S. Received March 29, 1888.

Most of the older and fairly complete analyses of plant-ashes disclosed the presence of alumina in sensible quantities. Gradually, however, as analytical methods became more exact, it was generally recognised that this constituent had been derived from extraneous sources and not from the plants themselves; alumina had in fact been introduced by the employment of glass and porcelain vessels, of impure reagents, and of imperfectly cleansed vegetable products. Even when traces of this oxide were obtained in analyses conducted under the most favourable conditions, an adventitious origin was assigned to them, and so the item of alumina disappeared entirely from the tables of the constituents of plant-ashes. Yet there were some conspicuous exceptions, although these were confined to certain cryptogams. For Ritthausen in 1851 ('Journ. Prakt. Chem.,' vol. 53, p. 413) found "much alumina" in the ash of *Lycopodium complanatum*, Linn., while Alderholdt in 1852 ('Ann. Chem. Pharm.,' vol. 82, p. 111) determined the percentage of alumina in the ash of the same Lycopodium to be 51·85 in the plant when gathered in March, and 57·36 when collected in November. The same chemist found 26·65 per cent. of alumina in the ash of *Lycopodium clavatum*. Again, in 1856, Solms-Laubach found ('Ann. Chem. Pharm.,' vol. 100, p. 297) in the ash of *L. clavatum* 27 per cent. and in the ash of L. complana-

*tum*, var. *Chamæcyparissus*, 54 per cent. of alumina. These results, with others by Arosenius, are conclusive as to the occurrence in notable proportion of alumina in the ash of certain Lycopodia. But when Solms-Laubach records in the ash of *Selaginella kraussiana*, A. Br. (erroneously described as *Lycopodium denticulatum*) the occurrence of 2 per cent. of alumina, we may regard the observation as likely to be incorrect; the same remark applies to the supposed discovery of a similar proportion of this earth in the ash of *Aspidium filix-mas* and of *Athyrium filix-fœmina*. And when the ashes of these plants were examined by modern methods, and with all the precautions which improved analytical processes require, then alumina can scarcely be recognised qualitatively in them. In one of the species of *Selaginella*, however, which I examined, I found a weighable trace of alumina, namely, 0·26 part in 100 parts of the ash. This plant, grown at Kew, was *Selaginella martensii*, var. *robusta* (the *compacta* of A. Braun). The ash was large in amount, namely, 11·66 per cent. in the dry plant; besides the 0·26 per cent. of alumina in it, there was 41·03 per cent. of silica ('Chemical News,' vol. 30, 1874, p. 137). In pursuing this inquiry, I examined, with every possible precaution to ensure exactness, three British species of *Lycopodium* all obtained from the neighbourhood of Shap in Westmoreland, as well as the single species of Selaginella which belongs to Britain. This last plant, now known as *Selaginella spinulosa*, A. Br., was formerly called *Lycopodium spinulosum*; my supply came from Largo Links in Fifeshire. The following figures represent the percentages which I obtained :—

| | Percentage of ash in dry plant. | 100 parts of ash contained | |
|---|---|---|---|
| | | $Al_2O_3$. | $SiO_2$. |
| *Lycopodium alpinum* ...... | 3·68 | 33·50 | 10·24 |
| *L. clavatum* ............. | 2·80 | 15·24 | 6·40 |
| *L. Selago*................ | 3·20 | 7·29 | 2·53 |
| *Selaginella spinulosa* ...... | 3·44 | none | 6·67 |

All these results pointed unmistakeably to the conclusion that while alumina was an important mineral constituent of many species of *Lycopodium*, it was practically absent from Selaginella. This distinction was confirmed by an analysis of *L. cernuum*, which I subsequently made. This species belongs to a group of the genus *Lycopodium*, quite distinct botanically from the group to which *L. alpinum* and *L. clavatum* belong, and also distinct from the *L. selago* group and the *L. complanatum* group. The following are the figures yielded by this plant :—

| | 100 parts of ash contained | |
|---|---|---|
| | $Al_2O_3$. | $SiO_2$. |
| **Lycopodium cernuum*, Linn. ...... | 16·09 | 30·25 |

I found alumina (qualitatively) in the ash of another member of the *L. cernuum* group, namely, *L. casuarinioides*, Spring, from Mount Ophir, Malacca, but the quantity of material at my disposal was too small to admit of quantitative determination. So far my results were strongly confirmatory of my conclusion that alumina was characteristic of *Lycopodium*, and absent from *Selaginella*. But this opinion was soon seriously shaken by an analysis of two exotic species of *Lycopodium*, namely, *L. Phlegmaria*, Linn., and *L. billardieri*, Spring. These plants were examined with the following results:—

| | Percentage of ash in dry plant. | 100 parts of ash contained | |
|---|---|---|---|
| | | $Al_2O_3$. | $SiO_2$. |
| **Lycopodium Phlegmaria* .. | 4·08 | 0·45 | — |
| **L. billardieri* ........... | 5·46 | trace | 3·14 |

On obtaining these results I abandoned the further prosecution of the inquiry, it being obvious that alumina could no longer be regarded as a characteristic ingredient of the ash distinguishing *Lycopodium* from *Selaginella*. But when Mr. J. G. Baker's work on the 'Fern Allies' was published last year I turned to the classification of the ninety-four species of *Lycopodium* there described, and found that these last-named plants belonged to a group containing eighteen species, all of which are epiphytic! It was clear that, having no direct access to the soil, these plants could obtain alumina only from their living hosts, which in all probability contained none or mere traces. The anomalous absence of this constituent from these two Lycopodia was thus in a measure explained; at all events, it was proved that alumina was not essential to all the species of this genus.†

The present research was extended by examining plants more or less closely related to the two genera under discussion. Following the classification of Sachs ('Text-book of Botany,' edited by S. H.

* The analyses, in the present paper, to which an asterisk is prefixed, have not been previously published.

† The occurrence of a high proportion of alumina in the mineral constituents of those coals which give the smallest proportion of ash loses much of its significance when the mode of the formation of coal is considered. It is impossible to feel sure that this ash is essential and not intrusive. The so-called Lycopods of the Carboniferous Period are, moreover, *now believed to belong* to the *Selaginaceæ*. Of course it is *possible that many of the plants of* that remote geological epoch may have *absorbed an element which their recent* representatives refuse.

Vines, 2nd Ed., 1882), we have *Equisetum*, *Ophioglossum*, *Salvinia*, and *Marsilea*, on one side of *Lycopodium*, with *Psilotum* and then *Selaginella* on the other—omitting, however, several families, including the true ferns. The results were negative.

| | Percentage of ash in dry plant. | 100 parts of ash contained | |
|---|---|---|---|
| | | $Al_2O_3$. | $SiO_2$. |
| *Equisetum maximum* .... | 20·02 | none | 62·95 |
| *Ophioglossum vulgatum* .. | 8·25 | none | 5·32 |
| **Salvinia natans* ......... | 16·82 | 1·86 | 6·71 |
| **Marsilea quadrifoliata* ... | 11·66 | 0·54 | 0·88 |

The alumina found in *Salvinia* was probably due to the presence of traces of soil from which it was found impossible to free this floating water-plant. Both the Salvinia and the Marsilea were grown in the lily house, Kew, and I have to thank the Director of the Royal Gardens for the material which I submitted to analysis.

The genus *Psilotum* has been mentioned as botanically near to *Lycopodium;* it contains but two species, one of which was examined for alumina with a negative result.

| | Percentage of ash in dry plant. | 100 parts of ash contained | |
|---|---|---|---|
| | | $Al_2O_3$. | $SiO_2$. |
| *Psilotum triquetrum* .... | 5·06 | trace? | 3·77 |

After *Psilotum* follows *Phylloglossum*, of which one species only has been recognised; this plant is too rare and too minute to be available for analysis; the same remark applies to the allied species *Tmesipteris tannensis*. *Selaginella* comes next, and then *Isoëtes*. An analysis of at least one of the species of this last-named genus is still a desideratum.

I will now revert, still following the classification of Sachs, to the true ferns. In none of the British species have I been able to detect more than traces of alumina. But among the exotic Cyatheaceæ which Sachs places above the Polypodiaceæ, there seems to be a notable exception. Last year Mr. W. F. Howlett, of Pahiatua, Wellington, New Zealand, forwarded to Mr. Thiselton Dyer some specimens of the ashes of a tree-fern. He wrote, under date 22nd February: "The other day I found a half-burnt *Punga*, or tree-fern. The ashes were pure white, very tenacious, and retained the structure of the wood. They were obviously not in any way contaminated with accidental impurities, nor had they been rained upon. . . . I wrote to a chemical student who said the ashes were chiefly alumina. This is very new to me. Alumina is generally thought an accident, here it cannot be so. I do not know the species of tree-fern."

Mr. Howlett's specimen of ash was handed to me by Mr. Thiselton Dyer; the following results were obtained on analysing it, every precaution being taken to ensure an accurate result:—

| | 100 parts of ash contained | | |
|---|---|---|---|
| | $Al_2O_3$. | $SiO_2$. | $K_2O$. |
| *Tree-fern, New Zealand........ | 19·65 | 12·96 | 15·1 |

This entirely unexpected discovery of nearly 20 per cent. of alumina (two determinations gave 19·8 and 19·5) in the ash of a tree-fern induced me to examine the ashes of known species of other Cyatheaceæ for this substance. Three specimens of the caudex of distinct species of these plants were furnished by the kindness of the Director of the Royal Gardens, Kew. Of these one only was sufficiently free from adventitious impurities to admit of trustworthy analysis. A cross section of the caudex of this plant, *Cyathea serra* from the West Indies, was sawn so as to preserve intact the whole of its pith as well as its fibro-vascular sheath. This section was broken up and burnt to a white ash, which amounted to 2·7 per cent. of the material dried at 100°. But it gave, on careful analysis, the merest trace of alumina.

| | Percentage of ash in dry plant. | 100 parts of ash contained | |
|---|---|---|---|
| | | $Al_2O_3$. | $SiO_2$. |
| **Cyathea serra*.......... | 2·70 | 0·20 | 12·65 |

Even this trace of alumina may have been extraneous, since the silica obtained was not entirely free from sandy particles (about $1\frac{1}{2}$ per cent. of the ash), although the material taken for the preparation of the ash was apparently perfectly clean.

Mr. Howlett forwarded, with the ash of the unknown tree-fern, a few grams of the caudex of a plant of *Cyathea medullaris*. The amount was quite insufficient for a satisfactory determination of the ash and its constituents, so I was obliged to content myself with a qualitative examination for alumina. The very small quantity of ash which I obtained on the incineration of these fragments of *C. medullaris* gave abundance of alumina. Indeed, I should not be surprised to find that the ash of the undetermined tree-fern was really that of this species of Cyathea. If this be the fact alumina will have been recognised, at present, in but a single species of tree-fern. Other genera of Cyatheaceæ, such as *Alsophila* and *Dicksonia*, may of course be characterised by the presence of this earth in notable quantities, but as yet analyses are wanting.

So far, it will be seen, alumina has been found in important quantities in a single tree-fern and in a number of different kinds of Lycopodium. The ash of another plant, however, contains over 2 per cen

of this earth. I refer, not to a vascular cryptogam, but to a member of the great class of *Musci*. In the water-moss, *Fontinalis antipyretica*, alumina occurs among the ash constituents in a proportion which seems too large to be quite accidental. The specimens which I analysed were obtained in May from the Thames and Severn Canal, near Cirencester. After having been thoroughly cleansed they were analysed with the following results:—

| | Percentage of ash in dry plant. | 100 parts of ash contained | |
|---|---|---|---|
| | | $Al_2O_3$. | $SiO_2$. |
| * *Fontinalis antipyretica* .. | 4·76 | 2·82 | 24·53 |

Further analyses of this plant and of its near allies are needed before a decisive conclusion can be drawn from this analysis.

In a previous paper, "Notes on the Occurrence of Aluminium in certain Cryptogams" ('Chemical News,' *loc. cit.*), I have detailed the various precautions which I have taken to prevent the intrusion of accidental traces of alumina during the analytical operations required for its determination. How far such precautions have been taken by the chemists who have recently investigated the occurrence of aluminium in certain vegetable products, I am not aware. But as the proportions of alumina obtained have been much smaller than those recorded by the earlier analysts, it may be assumed that the determinations are in general quite trustworthy. I now proceed to give a brief notice of the more important of these later inquiries, that it may be seen how their results differ from those to which attention has been directed in the present paper.

Mr. H. Yoshida found alumina in the ash of Japanese lacquer, the latex of *Rhus vernicifera* ('Chem. Soc. Trans.,' 1883, p. 481). But the quantity is quite insignificant. A tree yields annually about 2·5 grams of lacquer, and this contains from 3 to 8 per cent. of the gum in which alone the alumina occurs. Mr. Yoshida found 5·1 per cent. of ash in this gum, and on analysing its ash detected alumina in it to the extent of 6·3 per cent. or thereabouts ('Chem. Soc. Trans.,' 1887, p. 748). Now let us see to what amount of alumina this corresponds per tree, assuming the maximum amount, 8 per cent., of gum above-named to be present in the latex. A single tree yields—

| | | |
|---|---|---|
| Gum ................ | 0·2 | gram. |
| Ash in this gum........ | 0·01 | " |
| Alumina in this ash .... | 0·00063 | " |

*that is, a single tree annually yields rather less than two-thirds of a milligram of alumina.* In other words, the latex or lacquer contains *0·0025 per cent.* of alumina. The chief point of interest connected

with this fact seems to lie in the concentration of the alumina in the gummy matter contained in the latex. It should be remarked here that a little alumina occurs in the ash of some samples of cherry-tree gum and of gum arabic; whether this substance be constantly present remains to be ascertained.

Quite recently Mr. H. Yoshida ('Chem. Soc. Trans.,' 1887, p. 748) has determined the amount of alumina present in the ash of some grains and seeds, as *Glycine Soja*, the soy-bean; *Phaseolus mungo*, the Mung-bean (the var. *radiatus*); rice, wheat, barley, two species of millet and buck-wheat. The highest percentage, 0·272, was observed in the ash of Italian millet; the lowest, 0·053, in the ash of the soy-bean. In none of these cases can alumina be regarded as a characteristic ingredient.

Mr. W. C. Young ('Analyst,' vol. 13, 1888, p. 5) confirms Mr. Yoshida's results as to the occurrence of alumina in wheat. This experimenter found, moreover, that this constituent is intimately associated with the gluten. In Vienna flour, containing 0·7 per cent. of ash, he found 0·0075 per cent. of phosphate of alumina, which corresponds to 0·45 per cent. alumina in the ash. This proportion may be in excess of the truth, for, in separating the alumina strong sodium hydrate solution was boiled in a glass vessel, while no mention is made of a blank analysis having been made to control the result.

The quantity of alumina found by L'Hôte ('Comptes Rendus,' vol. 104, p. 853) in grapes and in wine seems to be too small to be taken into account; it is a mere trace.

So far as the materials at one's disposal warrant any definite conclusions, it may, perhaps, be permissible to say, that aluminium is a characteristic and abundant constituent of the ash of many, if not of all, the species of terrestrial Lycopodia; that it is absent from Selaginella and from a number of other allied vascular cryptogams; that it is present in notable quantity in at least one species of tree-fern though practically absent from others; and that it occurs in insignificant amount (like many other elements) in almost every plant in which its presence has been carefully sought for. As to the state of combination in which alumina exists in those plants in which it occurs in mere traces, we have very little information, but in the cereal grains and pulses it is probably in combination with phosphoric acid. In Lycopodia John states that aluminium acetate occurs, Ritthausen speaks of the malate Arosenius of the tartrate. Anyhow it is easy to extract abundance of an organic salt of aluminium by exhausting dried and pulverised *Lycopodium alpinum* with boiling water. So, in some cases, at least, the alumina present in these plants does not exist, as silica does in *Equisetum* and other highly silicious vegetable structures, in an insoluble form. As to the physiological function, if any, of this element, it is rash to offer an opinion. It is just possible that it may

serve to some extent to neutralise the abundant organic acids of the plants in which it occurs, and thus assist, like the cognate element magnesium, in the metabolic processes of vegetation.

One further observation may be hazarded. It remains to be seen whether the study of the periodic function which connects the atomic weights with the general properties of the elements will throw any light upon the relations subsisting between vegetation and the few elements necessary for its development. It seems that the position of aluminium in Mendelejeff's third periodic series decidedly favours the view of the peculiarity of its occurrence in certain plants, taken in the present paper. It stands between magnesium and silicon, two elements of which the physiological *rôle* is, to say the least, obscure; while of one of them—silicon, we may affirm that it is not an essential plant-food. Its occurrence in the ashes of various plants is indeed more general and more abundant than that of aluminium, but appears to be quite as capricious; and a point of difference as to the state in which these two elements are found in plants is obvious. Aluminium occurs mainly if not entirely in the form of soluble organic salts, silicon in the form of insoluble silica.

In considering this aspect of the periodic law one cannot help being struck with the low atomic weights of the essential elements of plants. If we exclude certain cases of apparently casual and accidental absorption (of such elements as bromine, iodine, copper, zinc and arsenic) it will be noticed that Mendelejeff's Series I, II, III and IV, having a range of atomic weights from 1 to 56, comprise all the essential elements, even if we include manganese, chlorine, silicon and

Elementary Plant-Food and the Periodic Law.

| Series I. | Series II. | Series IV. |
|---|---|---|
| | (Lithium = 7·0) | POTASSIUM = 39·1 |
| | (Beryllium = 9·1) | CALCIUM = 40·0 |
| | (Boron = 11·0) | (Scandium = 44·0) |
| | CARBON = 12·0 | (Titanium = 48·1) |
| | NITROGEN = 14·0 | (Vanadium = 51·3) |
| | OXYGEN = 16·0 | (Chromium = 52·3) |
| | *Fluorine* = 19·0 | *Manganese* = 55·0 |
| HYDROGEN = 1. | | IRON = 56·0 |
| | *Sodium* = 23·0 | |
| | MAGNESIUM = 24·0 | |
| | *Aluminium* = 27·1 | |
| | *Silicon* = 28·3 | |
| | PHOSPHORUS = 31·0 | |
| | SULPHUR = 32·0 | |
| | *Chlorine* = 35·5 | |
| *Series I.* | Series III. | |

aluminium. The identity of the position occupied by fluorine in Series II with that of manganese in Series IV perhaps admits of correlation with the occurrence of these elements in plants.

The table (p. 128) illustrates the preceding observations, and shows the periodic position of aluminium—the element primarily under discussion. For the sake of distinctness the elements generally believed to be essential to the higher plants are printed in capitals, the elements of doubtful necessity in italics, and those which, if they occur at all in plants are certainly accidental, in ordinary type enclosed in brackets.

*Postscript.*—Since writing the above paper I have found that the ash from the caudex of another tree-fern (*Alsophila australis*) contains a very large quantity of alumina. The specimen analysed was from Tasmania. I have also detected more than mere traces of alumina in the ash of the caudex of *Dicksonia squarrosa.*

## IV. "On the Nature and Limits of Reptilian Character in Mammalian Teeth." By H. G. SEELEY, F.R.S., Professor of Geography in King's College, London. Received April 4, 1888.

Approximations between reptiles and mammals have been recognised in many parts of the skeleton.* They are most marked between certain genera and orders of the two classes. The oldest known fossil representatives of both groups certainly approximate closer towards each other in all known parts of skeletons than do the orders which survive; so it may be a legitimate induction that, in an earlier period of geological time, the characters of both groups were so blended, that there existed neither the modern reptile, which has specialised by losing mammalian attributes, nor the modern mammal, which has specialised by losing the skeletal characters which have come to be regarded as reptilian. The most ancient mammals exhibit, in the known parts of their skeletons, resemblances to Monotremes, Edentates, Insectivores, and apparently Carnivores; and it is among these orders that the closest correspondence is found, bone for bone, with reptiles. Therefore, if an attempt were made to predict on an inductive basis, the kind of dentition which the earliest mammals which existed would show, it might be expected to be in harmony with the mammalian and reptilian characters of their skeletons. On the same basis it might be suspected that existing mammals, with

* "*Resemblances between the Bones of* typical living Reptiles and the Bones of *other Animals;*" "*Similitudes of the Bones, &c.,*" 'Journal of the Linnean Society, *Zoology,' vol. 12, 1874, pp. 155, 296.*

reptilian elements in the skeleton, would still preserve teeth which might be compared with teeth of reptiles; and as a matter of observation it is found that there are several features in which teeth of reptiles and mammals resemble each other morphologically.

The idea conveyed by the expression "mammalian tooth" is necessarily that specialisation of tooth structure which is limited to the mammalian class. It may be unknown in the dental conditions of entire families and orders of mammals. And there is an absence of pronounced character in the incisor or canine teeth of any mammal order which would distinguish them as mammalian.

Similarly the idea implied in the term "reptilian tooth" is the specialisation of teeth in the reptilian class, which is as far from being universal in the class, as mammalian teeth are universal among mammals. Indeed, the lower mammals emphatically approach towards reptiles in all essential characters of tooth form.

Because the diversities in the teeth of the two classes have been emphasised for purposes of classification, the significance of the resemblances has been less considered.

There are six typical characters of teeth which are regarded as mammalian. They are:—

(1.) The presence of more than one root to a tooth;
(2.) The implantation of teeth by distinct sockets;
(3.) The existence of different kinds of teeth in the same jaw;
(4.) The development of distinct cusps to the teeth;
(5.) The wear of the crown with use;
(6.) Replacement by a successional series;

No one of these characters can be relied on as constant in the class; and its loss is in every case an approach towards a reptilian type.

First, the root is not the original or essential part of the tooth. While the sucessional teeth are within the jaw they commonly have the roots undeveloped, and thus up to a certain stage of growth are without this evidence of class character. There is never more than one root to an incisor or canine tooth in any mammal; and never more than one root to any tooth (so far as I can ascertain) in an existing Edentate or Cetacean. Hence if all mammals are supposed to have had a common origin, it is legitimate to conclude that all the teeth originally possessed but one root; and that there is a certain relation subsequently established between the complexity of the crown and the number of the roots.

The situation of a root would imply that its development is due to the same law of growth under intermittent pressure or strain as determines the form or elongation of any other bone.* If more than one root is present they are commonly beneath the several parts of a tooth which have to resist intermittent strain or pressure. If the pressure

* "The Mechanism of Growth," 'Ann. Mag. Nat. Hist.,' April, 1872.

is great and the wear considerable the crown of the tooth grows in length, while the roots are relatively small; but if the intermittent strain on the tooth is great then the crown is relatively short and the roots long. The latter condition is well seen in the molars of Carnivora; the former in the molars of rodents and ungulates. The small roots of ungulates and rodents illustrate a mode of development of roots: for I have seen teeth of an aged fossil horse from the gravel in which the crown was completely worn down, and then the roots appeared to be relatively almost as well developed as in *Rhinoceros*.* Perhaps no order is more instructive in regard to the classificational value of roots of teeth than the Sirenia, because *Manatus* has tuberculate teeth and well-developed roots to the molars, while *Halichore* has but one strong root to these teeth, indistinguishable from the crown, with a hollow conical base, such as is often seen in Reptiles. From these considerations I infer that the type of tooth—at least as regards complexity—is to be correlated with the influences exercised by food, and is not a distinctive inheritance.

Secondly, the implantation of teeth in bony sockets is a mammalian character which is not less well marked in the Crocodilia and some extinct orders of Reptiles. The implantation in mammals with single roots to the molars differs in no way from the conditions which I have observed in Theriodont Reptilia. There are some exceptions among mammals to the location of teeth in sockets, since in certain Cetacea the teeth are in a groove at the posterior end of the series. And the Ornithorhynchus may be regarded as another exception, since it has three teeth on each side closely united together into one long ovate mass which is contained in a groove. The teeth are closer together than those of *Ichthyosaurus*, and there is no more definition of the groove into separate sockets than in that genus; but there is nothing else in common, since the base of the dental plate of *Ornithorhynchus* can scarcely be said to have roots. Frederick Cuvier described these teeth as horny,† and many writers have been disposed to regard them as horny plates rather than true teeth. Sir R. Owen quotes a French analysis of the tooth substance as yielding 99·5 horny matter and 0·3 calcareous matter.‡ This may be true of the long anterior horny plates on the jaws, but it can hardly apply to the posterior teeth which are in a socket-groove. If the dental plate is extracted from the jaw and examined against transmitted light, each of the three teeth which form it will be seen to consist of a large opaque subquadrate central portion, and an external translucent border of a horny appearance. I regard the latter as representing the uncalcified enamel of the tooth, while the central portion corresponds to the

* The specimen was obtained by the Rev. N. Brady from near Cambridge.

† 'Des Dents des Mammifères,' 1825, p. 203.

‡ 'Odontography,' p. 311.

remainder of the tooth. I have had an opportunity, by the kindness of Dr. Garson, of examining the microscopic sections of these teeth prepared by the late Professor Quekett, and preserved in the Museum of the Royal College of Surgeons, and they confirm my previous impression that the central portion of the tooth is bony (at least in some specimens), and in microscopic structure it shows large haversian canals surrounded by spaces and canaliculi. I therefore regard these teeth of *Ornithorhynchus* as true teeth. But they seem to me to be teeth in course of degeneration, and in process of losing their calcareous matter. They have already lost their root or roots, and have partially lost their individuality. The long anterior dental ridges appear to have carried this change one step further and have become dental layers formed of vertical parallel plates of horn in which there is no division into separate teeth, which are not imbedded in the jaw, but are a horny superficial substance. It is not without interest to remark that some other animals which have lost their teeth, like birds, and presumably Chelonians, which use the jaws for biting, also have them sheathed in horn; for the condition in *Ornithorhynchus* suggests that the horny substance may represent the lost substance of teeth.

Thirdly, mammalian teeth are commonly distinguishable into different kinds, which when fully developed vary in the forms of their crowns, and are thus recognised as incisors, canines, premolars, and molars. This differentiation is almost entirely absent from the dentition of Cetacea and Edentata; and it is well known that in different orders, canine teeth, or incisor teeth, or both, may be absent. These conditions can be frequently correlated with food. But just as the grouping of the teeth in mammals may approach in simplicity the condition in reptiles, so the teeth of some reptiles in different parts of the jaws may parallel the divisions found in the jaws of mammals which show considerable differentiation.

The fourth mammalian character is the cuspidate condition of the crown of the tooth. This results from a folding of the substance out of which the tooth is formed, and among the molar teeth of many mammals shows a specialisation which is unparalleled among reptiles. But on the other hand the complexity of some hinder-molars becomes simplified in the premolar region, and among Edentates and Cetaceans the tooth crowns are simpler than among some reptiles. In several orders of mammals it is obvious that the direction in which the folds of tooth substance are disposed is at right angles to the direction of movement of the lower jaw; and therefore it may be a fair inference that the transverse widening of molar teeth, no less than their diverse *cuspidate character*, is to be attributed to the increased work which *food has given them* to do in the molar region; and that development or *suppression of* a cusp in allied genera of mammals depends upon this

cause. With simplicity of function there is simplicity of detail in the crown of the tooth. Some of the simpiest teeth are found among the Edentata, where the tooth is often sub-cylindrical, but as the crown is worn down, its original form is not seen. *Tatusia,* however, is an Edentate with successional teeth, and while the crown is still within the jaw it has a form which is as reptilian in aspect as the molar tooth of a *Teius.* The crown of the tooth of a Cachalot is a short curved cone. Hence it is manifest that the molar teeth of mammals are not necessarily cuspidate, and that in simplicity of crown there may be no character to distinguish a mammal from a reptile. From which it is probable that some primitive fossil mammals may also have a reptilian type of dentition. The recent discovery of a set of teeth in the jaws of *Ornithorhynchus,* hitherto unknown, raises the question whether those teeth are mammalian. Mr. Poulton has only contributed a vertical transverse section of one of these teeth,* which shows elevated external and internal cusps. I have no other knowledge of those teeth, but the condition figured is suggestively similar to a corresponding section of a molar tooth of the lizard genus *Teius.*† Professor Mivart quotes‡ from Mr. Poulton a passage, which I do not find in that gentleman's paper, describing the tooth, and from that description it would appear to correspond generally with the tooth of the adult *Ornithorhynchus.* I have already considered some characters of those teeth, and allowing for their degeneration, they seem to me to approach as close perhaps to the form of crown in lizards like *Teius* as to any of the larger bats.

Fifthly, mammalian teeth are often remarkable for the wear of the crown. This attrition appears to depend upon the form of the crown, the apposition of crowns, the development of enamel, and the nature of food. It is exceptionally well seen among Elephants, Ungulates, and Edentates; but almost all mammalian teeth show some change of aspect with wear. This condition is much less general among reptiles; but in the extinct Ornithischia the serrated crowns of the teeth are as well worn as in any mammal. The long teeth of *Hyperodapedon*§ appear to be well worn down to the palate. Exceptionally teeth of *Ichthyosaurus* and *Polyptychodon* show both vertical wear and lateral wear, and there are specimens in the Woodwardian Museum from the Cambridge Greensand in which teeth of these genera have the crown worn away transversely almost down to the root; so that neither wear nor its absence has any importance as a class character,

* 'Roy. Soc. Proc.,' vol. 43, p. 355.

† Sir R. Owen compares the teeth of *Ornithorhynchus* to those of the reptilian fossil *Placodus* ('Geol. Soc. Quart. Journ.,' vol. 36, p. 423), but the details of structure of the crown are not the same.

‡ 'Roy. Soc. Proc.,' vol. 43, p. 373.

§ Lydekker, 'India Geol. Surv. Mem.,' ser. iv, vol. 1, part 5, pl. 2.

but this condition of teeth varies in every order with the habitual food.

Finally, the succession of the teeth has been regarded as a mammalian class character. It is exceptional, and an individual peculiarity, for more than two sets of teeth to be cut in a mammal, though evidence has been brought forward that this reptilian condition is occasionally present in man. But even in those mammals which cut a second set of teeth there are commonly some molars which have no predecessors, and are a single series throughout life. So far as is known, most Edentata and Cetacea have but one set of teeth, which is never renewed; and according to Professor Flower, *Tatusia* is the only Edentate in which successional teeth are known to be developed. I have seen no evidence of a successional tooth in any Dicynodont reptile. Sir R. Owen has found no evidence that the Theriodontia possessed "a milk series of teeth."* When a successional tooth is present in mammals it usually originates below the tooth in wear, or behind it as in the elephant. This condition is seen in some reptiles as in the Ornithischia. But the typical condition of reptilian succession is for the germ of the new tooth to be on the inner side of the tooth in wear. This is the condition in Ichthyosaurs and most of the extinct Reptilia, and is often though not invariably seen in Crocodiles. It is, therefore, interesting that Mr. Poulton describes the new-found teeth in *Ornithorhynchus* as possibly on the inner side of the so-called horny plates, though in the lower jaw they are certainly below those plates. Hence, if those germs are successional teeth their relative position would not be inconsistent with reptilian or mammalian type.

From this discussion I conclude that in all morphological relations the teeth of mammals may be so simplified as to approach closely to conditions which would be regarded as typically reptilian.

I have next to show that the prevalent conception of the reptilian type of tooth is equally indefinite. The differentiation is less striking than among mammals, but in almost all morphological characters reptiles suggestively approach mammals, though these characters seem to me most remarkable in the grouping of the teeth into analogues of molars, premolars, canines and incisors, and in the characters of the crown in molar and other teeth. It is rather among the oldest extinct Reptilia that we should expect to find the nearest approach to mammalian dentition, and so it is; but evidences of a similar differentiation may be detected among Crocodiles and Lizards.

The form of the crown varies very little from front to back among Crocodiles, though some teeth are relatively large, and the smaller *posterior teeth* are a little compressed transversely; but when the *teeth are drawn* from the jaw the alveoli show modifications which

* 'Geol. Soc. Quart. Journ.,' vol. 37, p. 261.

might be regarded as mammalian. This character has been figured from the lower jaw, and in 1878 it was remarked* "among Crocodiles, I recognise in the well-known wavy outline of the jaws a demarcation of teeth into regions which have a fair right to be named incisors, canines, premolars, and molars, and constitute a dentition as Theriodont in principle, but not so specialised, as is seen in the South African fossil group. In the Crocodile the regions are easily recognised by the form, size, and characters of the tooth sockets when all the teeth are drawn, especially in the lower jaw. The incisors occupy a flat or slightly concave region below the premaxillary bone. Then at the head of the crest is the large canine placed between the premaxillary and maxillary bones. Next succeeds a portion of jaw with concave outline occupied by small teeth, which sometimes become larger from before backward; these are the premolars. And, lastly, there are teeth in another concave region which have the position of molars; these may, in the young animal, all be contained in a groove, with sockets scarcely better indicated than among Ichthyosaurs or some Cetaceans. In many Teleosaurs and Plesiosaurs the incisor teeth are relatively large, and the succeeding molars are smaller; and in the Ornithosaur *Dimorphodon* the incisor teeth are exceptionally large, as compared with the molars. The teeth of South African reptiles termed Theriodontia differ from such types chiefly in the development of large canines. The incisors remain large, the canines are larger, and the molars relatively small, as may be seen in such genera as *Cynodraco* and *Lycosaurus*. In this group the incisors have both crown and root compressed from side to side. The crown often has a prominent sharp chisel-like external cusp, and a small internal cusp which gives the tooth a mammalian aspect. This character is well seen in the Russian genus *Deuterosaurus* as figured by Eichwald and by Mr. Twelvetrees, the latter specimen being in the National Collection. A similar condition, but with the inner cusp less conspicuous, is seen in a new genus from South Africa allied to *Deuterosaurus*, here figured, which may be named *Glaridodon*. In this tooth, besides the elevated outer and inner cusps, there are on both sides elevated lateral borders to the crown, so that it includes a concave area, which in mode of formation of the concavity may be compared to the concave crown of the molar tooth of *Ornithorhynchus*, though the proportions of the tooth are dissimilar. Yet if a tooth of this type is supposed to lose its root by degeneration, it might show a close approximation to the tooth of such a mammal as *Ornithorhynchus*. The canine teeth in Theriodonts, like those of some of the lower mammalian orders, appear to be *placed in the* maxillary bone, and not in the suture *between that bone and the* premaxillary, as in the higher mammals.

* "*On Procolophon*," 'Geol. Soc. Quart. Journ.,' vol. 34, 1878, pp. 804-5.

[redacted]



FIG. 1.

Lateral aspect of an incisor tooth, *Deuterosaurus*, Brit. Mus., R. 303.

FIG. 2.

Lateral aspect. Posterior aspect.

Crown.

Root.

*Incisor* tooth, *Glaridodon*.—Brit. Mus., 49425.

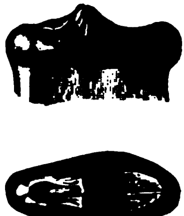
Richard Owen has shown, these teeth in size, form, and serra-
:e altogether like canines of carnivorous mammals. The molar
of Theriodonts are usually but little specialised, and are small
'ten simple cones. Even in *Galesaurus* the crowns of the molars
›mpressed from side to side, and they have a central cusp no
developed than in a lizard, with a smaller cusp on each side,
as in some seals and porpoises, and as among porpoises there is
single root.

Fig. 3.

and canine teeth of *Galesaurus.*—Brit. Mus., R. 845. The posterior teeth
are fractured, showing that the pulp cavity is closed at the base.

American genus, *Empedias*, from Permian or Triassic rocks,
ed by Professor Cope to a distinct order, the Pelycosauria, shows
ɩusual specialisation of the molar teeth. They are compressed
front to back, so as to have a great transverse extension on the
, which is absent from the premolars. There is a contraction
the crown which is quite mammalian, and the root is single.
:rown may be described as having three cusps. The median

Fig. 4.

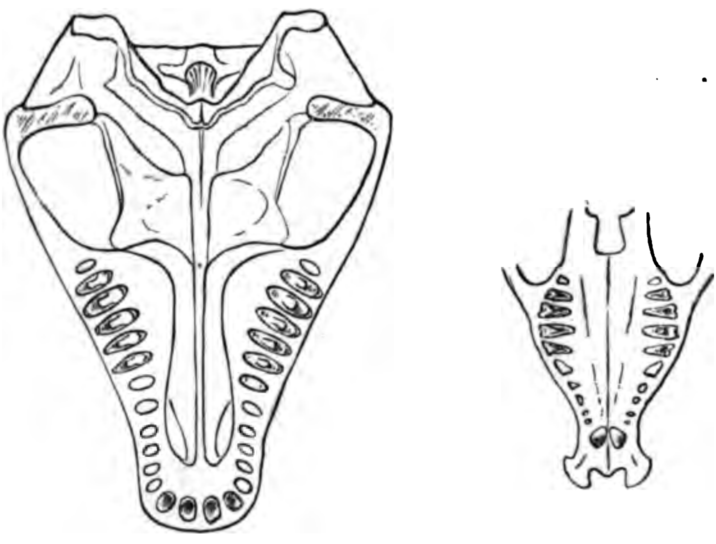


ɪverse and superior views of molar tooth of *Empedias.*—Brit. Mus., R. 613.

l cusp is the most elevated, and is the only one which shows
ɩce of wear, but the external and internal limits of the crown
th elevated above the level of the concave spaces which divide
from the middle cusp. Hence the tooth offers some evidence of
cusps in parallel series as a reptilian character, and so far helps
*roximate reptilian and mammalian* types. This dental con-
ɪ *Empedias has its chief* interest in an approximation which

it makes to the Golden Cape Mole, *Chrysochloris aurea.* Its teeth ; rather more numerous in the premolar region, but otherwise t molars in the mammal similarly have one root; they have the sa transverse extension with three cusps, of which the middle one similarly well-developed, so that the chief differences are that *Chrysochloris* the crown is wide on the outer margin and narro internally as a wedge, while the external cusp is subdivided into tw The lower jaw teeth of *Empedias* resemble those in the skull, but : *Chrysochloris* the mandibular teeth are bicuspid, except that the fir two molars have the inner cusp divided longitudinally. In tl accompanying figures these genera are contrasted; and if *Galesaur* suggests a primitive mammalian type allied in dentition to sea *Empedias* as strikingly resembles an insectivorous mammal.

FIG. 5.



*Empedias molaris.*
Reptilian dentition.

*Chrysochloris aurea.*
Mammalian dentition.

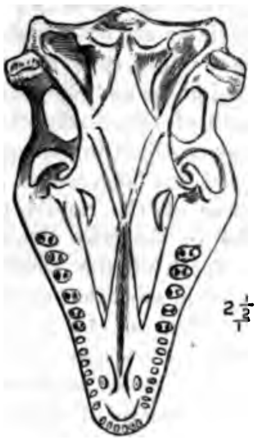
The Lacertilia include many types of dentition, among which : genera with characters suggestively mammalian both in the group: of the teeth and forms of the crowns.

In the Frilled Lizard, *Chlamydosaurus*, there is one canine tooth each anterior angle of the lower jaw, and these teeth are separa *from each other* by small incisors. In the skull there are on ea *side in corresponding* positions two canine teeth placed side by s *laterally in succession* to each other.

In most lizards, as in many mammals, canine teeth are absent; and sometimes there is a more or less marked gap in the positions in which they might occur.

The teeth which are in the position of molars may exhibit modifications in the forms of the crown which correspond to premolars and molars. Thus, in species of *Teius*, there are five or six bicuspid teeth which have the cusps one internal to the other,* while in front of them

FIG. 6.




Palate of *Teius*, showing bicuspid molars, premolars with one cusp, and incisors.—After a photograph by Herbert Jackson, Esq.

FIG. 7.

A molar tooth of *Teius*, seen from above, much enlarged.

* I have on more than one occasion inadvertently attributed this character to the genus *Cnemidophorus*, as my specimen was so labelled when it came into my possession. I am indebted to Mr. Boulenger for the rectification, and whenever I have referred to the character it should be associated with the genus Teius.

are about seven teeth with single cusps which correspond to the oute cusps of the posterior part of the series. In this genus there is a longi tudinal channel between the cusps of the molar teeth. Seen from th palatal aspect the crown of a tooth is sub-quadrate, and the external cusp is the more elevated, so that the tooth has an aspect which is insectivorous rather than edentate. Both cusps are compressed so as to form sharp longitudinal cutting edges. At their bases they are connected on both the anterior and posterior borders of the tooth by low transverse concave ridges. In my specimen these transverse ridges are sufficiently marked in the skull; but are stronger in the lower jaw, where their surfaces are not quite smooth. If the anterior and posterior ridges were stronger, the crown of this tooth in quadrate form, external and internal cusps and elevated border, would be sufficiently similar to the tooth of *Ornithorhynchus* to give some ground for regarding that tooth as reptilian in plan. And it has already been seen that in degeneration of the fang, which induced Sir R. Owen to compare the teeth to those of the reptile genus *Placodus*, and in implantation in a groove in the jaws there is no departure from reptilian types. If the tooth of the *Ornithorhynchus* as a whole cannot be exactly paralleled in any other animal, it is at least evident that the teeth are as reptilian as the skeleton; and if the correspondence is not closer, the reason may be found in the degeneration which has replaced the enamel of the tooth with horny matter.

Modern lizards are not a group of animals in which theoretical considerations would suggest a search for mammalian characters in the teeth, but they happen to be the only group of Reptilia which is at all well known in which the teeth show a diversity which is in any degree comparable with the diversity of mammalian teeth. Whether those characters have been inherited from remote ancestry, or spontaneously developed in their possessors under varying conditions of existence, as seems probable, is a matter of small moment, for in either case they throw illustrative light on the classificational value of teeth of mammalia. If the different forms of cusp development found in lizards could be combined, teeth would result with crowns like the cuspidate crowns of many mammals. Thus, in *Cnemidophorus* there are two cusps arranged longitudinally; in *Ameiva* the tooth may have one large cusp with a small cusp by its side, or in the fore-part of the jaw there may be a small cusp on each side. If this kind of serration were combined with the transversely bicuspid teeth of *Teius* or of *Empedias* crowns would result which would have mammalian patterns. In *Amblyrhynchus cristatus* the external part of the crown is deeply cleft so as to be divided much as in some seals into a median denticle, flanked by a lateral denticle on each side; but on the internal side the base of the crown thickens, forming a large flattened oblique area, which is evidently an undivided internal cusp, like the intern

incisor cusp of *Deuterosaurus*, for it is equally developed in successional teeth which have not come into use. Thus, *Amblyrhynchus* makes a partial combination of the characters of *Ameiva* and *Teius*, and shows what may be termed a sub-mammalian type.

FIG. 8.

Anterior aspect. Lateral aspect. External aspect.


Teeth of *Amblyrhynchus*. Two molar teeth of *Ameiva*.

The teeth of *Iguana* are serrate and acuminate, but if they were supposed to lose the acuminate character by all the denticles growing to the same height from a depressed base, then the parallel vertical serrations would reproduce the incisors of *Galeopithecus;* and that the incisors have originated in some such way is suggested by the premolars in that genus being acuminate and serrated. The grooved tooth of *Plagiaulax* and *Hypsiprymnus* is equally suggestive of the origin of complicated molars from a simpler form such as may be found in reptiles. It is well to remember, as showing how difficult it is to recognise class characters in the form of a tooth crown, that a naturalist so familiar with mammals as de Blainville was of opinion that the small mammalian jaws from Stonesfield, known as *Amphitherium*, were the jaws of reptiles before Sir R. Owen demonstrated that the molar teeth possessed two roots. But whether the molar teeth of mammals were evolved out of simple reptilian types of teeth such as have been discussed as consequences of other changes in the skull, or are due to the influence of habitual food on inherited structure, it is to be anticipated that the primitive mammals possessed teeth of reptilian type, less differentiated than the molar teeth of some existing lizards.

V. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. IV. On a Large Humerus from the East Brak River, South Africa, indicating a New Order of Fossil Animals which was more nearly intermediate between Reptiles and Mammals than the Groups hitherto known." By H. G. SEELEY, F.R.S. Received April 5, 1888.

(Abstract.)

The late Mr. A. G. Bain sent to the British Museum a bone, No. 36,250, which the author regards as a right humerus. It is 32 cm. long. The crests at its proximal end are compared with those in the corresponding bone of Saurischia, Ornithosaurs, and Anomodonts; and they show a strong general resemblance to the crests seen in Monotremes, though their direction may be more reptilian. The distal end of the bone is entirely mammalian in plan. Its resemblances are about equally strong to Edentata and Monotremata, and there are evidences of more distant relationship with Insectivora, with certain Marsupials, seals, and other Carnivora. On the whole the evidence is insufficient to refer the fossil to the Monotremata. It is named *Propappus omocratus.* The author proposes to associate with it *Stereorachis* of Professor Gaudry, in an order named Gennetotheria. While the humerus of *Stereorachis* only differs from Monotremes in generic characters, and conforms in plan to the monotreme rather than the edentate type, the shoulder-girdle is intermediate between Echidna and the Anomodont *Keirognathus*, and the dentition resembles that of reptiles like *Lycosaurus* and other Theriodonts.

VI. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. V. On Associated Bones of a Small Anomodont Reptile (*Keirognathus cordylus*, Seeley), showing the Relative Dimensions of the Anterior Parts of the Skeleton, and Structure of the Fore-limb and Shoulder-girdle." By H. G. SEELEY, F.R.S. Received April 5, 1888.

(Abstract.)

This specimen was collected by Mr. Thomas Bain at Klip Fontein, Fraser's Berg, and registered in the British Museum as 49,413.

*The head* is described in detail, and except in the very small size *of the teeth*, shows no difference of importance from the skulls attributed to *Dicynodon.*

The shoulder-girdle is described and restored, and found to consist of interclavicle, clavicles, sternum, coracoids, pre-coracoids, and scapulæ. The scapula is in plan like *Kistecephalus*. The nearest approach to the coracoid and pre-coracoid is found among the monotreme mammals. The clavicle extended along the anterior margin of the scapula, and made an angular bend, so as probably to meet the interclavicle. The interclavicle appears to meet the lateral margins of the coracoids and not to overlap them in front. It approximates in form to the bone in *Ornithorhynchus*, *Ichthyosaurus*, and certain lizards, but is relatively much larger, and is larger than the interclavicle of *Stereorachis*. The sternum, which is transversely extended, is better compared with that of a lizard or Dinosaur. The shoulder-girdle as a whole is intermediate between monotreme mammals and known reptiles, but with the former type predominating.

The bones of the fore-limb are described in detail, and found to be relatively long and slender and generically unlike *Dicynodon*. The carpus is complicated. There are only two phalangeal bones in each digit, the second bone being a well-developed claw.

Finally a restoration is given of the aspect of the animal.

## VII. "On the Modifications of the First and Second Visceral Arches, with especial Reference to the Homologies of the Auditory Ossicles." By HANS GADOW, Ph.D., M.A., Strickland Curator and Lecturer on Comparative Anatomy in the University of Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. Received April 12, 1888.

(Abstract.)

The phylogenetic development of the first visceral arches shows us some most interesting changes of function, which we can follow upwards from the lower Selachians to the highest Mammals.

Originally entirely devoted to respiration as gill-bearing structures, the whole hyoidean arch becomes soon a factor in the alimentary system. Its proximal half forms the hinge of the masticatory apparatus, its distal half remains henceforth connected with the process of deglutition. Then this suspensorial arrangement is superseded by a new modification; the hyomandibula is set free and would disappear (it does nearly do so in Dipnoi and certain Urodela), unless it were made use of for a new function; with its having entered the service of the conduction of sound, it has entered upon a new departure, and it is saved *from degeneration*. The whole system of the one to four *elements of the middle ear*, which all have the same function as *conductors of sound, is to be looked* upon as *one* organ of one common

origin, namely, as a modification of the hyomandibula, the primitive proximal paramere of the second visceral arch.

*Successive Modifications of the Mandibular and Hyoidean Visceral Arches.*

I. Primitive condition (Notidanidæ). The palato-quadrate bar alone carries the mandible. The second arch is indifferent. Hyomandibula and quadrate (the palatine part is an outgrowth) are both attached to the cranium.

II. The hyomandibula gains a fibro-cartilaginous connexion with the mandible, the masticatory apparatus becomes amphistylic and occasionally hyostylic (Rajidæ, most Selachians).

The hyoid gains a cranial attachment (many Rajidæ).

III. The quadrate- or autostylic suspensorium becomes preponderant; the hyomandibula is, as in Teleosteans, divided into a proximal and into a distal (symplectic) element. The proximal part is received into a fenestra of the otic capsule, and is converted into a stapes, whilst the distal half either remains (*Proteus*, *Siren*, *Menopoma*) or is lost (other Urodela). The whole hyomandibula would have been lost owing to its excalation from suspensorial functions, unless it had entered the auditory service.

IV. The autostylic arrangement prevails. The whole hyomandibula remains, gains an attachment on the "tympanum," and differentiates itself into several conjointed pieces, notably stapes or columella proper, and extra-columella or malleus.

The extra-columella gains connexion with the parotic cartilage; this connexion frequently remains, but in *Anura* alone it contains a special element of probably parotic origin.

The quadrate forms an important part of the tympanic frame.

IV*a*. Collateral departure of the *Anura*. The connexion between the tympanal part of the hyomandibula with the mandible is lost.

V. The quadrate still forms the principal suspensorial part of the mandible. The extra-columella, or malleus, retains for a long time its previously acquired connexion with Meckel's cartilage (*Amniota*).

V*a*. The top end of the hyoid is attached to the cranium (*Geckos*, *Mammalia*), and is occasionally fused with the extra-columella (*Hatteria*).

V*b*. Or, the proximal portion of the hyoid is removed from the skull and remains otherwise well developed (*most Lizards*); or its proximal portion becomes reduced and lost (*Chelonia*, *Crocodilia*, *Ophidia*, *Aves*).

V*c*. The extra-columella gains an attachment to the quadrate, *squamosal*, or pterygoid, whilst its connexion with the mandible (*Ophidia*, *Chamæleon*), and the tympanum, is lost.

*VI. The quadrate* gradually loses its articulation with the mandible; *the latter* gains a new outer articulation with the squamosal;

the quadrate acts almost entirely as a tympanic frame. Incus and malleus fuse sometimes with each other, and lean on to the parotic region. The masticatory joint is doubly concave-convex (*Monotremata*).

VII. The quadrate is converted into the principal part of the tympanic frame, viz., annulus tympanicus. The mandible has lost its articulation with the quadrate, and the masticatory joint is a single concave-convex one, the convexity belonging to the mandible (*Monodelphia*).

*Presents, April* 26, 1888.

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*May* 3, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Andrews, Thomas, F.R.S.E.
Bottomley, James Thomson, M.A.
Boys, Charles Vernon.
Church, Arthur Herbert, M.A.
Greenhill, Professor Alfred George, M.A.
Jervois, Sir William Francis Drummond, Lieut.-Gen. R.E.
Lapworth, Professor Charles, LL.D.
Parker, Professor T. Jeffery.
Poynting, Professor John Henry, M.A.
Ramsay, Professor William, Ph.D.
Teale, Thomas Pridgin, F.R.C.S.
Topley, William, F.G.S.
Trimen, Henry, M.B.
Ward, Professor Henry Marshall, M.A.
White, William Henry, M.I.C.E.

The Right Hon. John Hay Athol Macdonald, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On the Induction of Electric Currents in conducting Shells of small Thickness." By S. H. BURBURY, M.A., formerly Fellow of St. John's College, Cambridge. Communicated by H. W. WATSON, D.Sc., F.R.S. Received March 22, 1888.

(Abstract.)

1—4. Definition of current sheets, current shells, superficial currents, and current function.

5. Expression for the vector potential of the currents in a sheet.

6. *Expression for the energy* of a system of current sheets in terms *of the current function and magnetic* potential, viz.:—

$$2T = \iint \phi \frac{d\Omega}{d\nu} dS,$$

where $\phi$ is the current function, $\Omega$ the magnetic potential, and $d\Omega/d\nu$ the rate of its variation per unit of length of the normal.

7. The magnetic induction due to the sheet with current function $\phi$ is the same as that due to a magnetic shell of strength $\phi$ over the surface at all points not within the substance of the shell.

8. Given any magnetic field external to a surface, S, there exists a determinate system of magnetic shells over S having at all points within the surface magnetic potential equal to that of the externa field.

9 and 10. Therefore also a system of currents over the surfac having the corresponding property, called the *magnetic screen* Example of a sphere.

11, 12, and 13. If the function $\psi$ satisfy the conditions

$$d\psi/d\nu = lF + mG + nH \quad \text{on S},$$

$$\nabla^2\psi = 0 \quad \text{within S},$$

then $F = d\psi/dx$, &c., if F, G, H be the components of vecto potential due to the external system and its *magnetic screen.* $\psi$ i called the companion function to F, G, H.

14—17. Solution of the problem of induction in the absence o resistance by Lagrange's equations, where the external system varie continuously, in the form—

$$\frac{d}{dt}\frac{dT}{d\phi} = 0,$$

where
$$2T = \iint \phi_0 \left(\frac{d\Omega_0}{d\nu} + \frac{d\Omega}{d\nu}\right) dS_0$$
$$+ \iint \phi \left(\frac{d\Omega_0}{d\nu} + \frac{d\Omega}{d\nu}\right) dS,$$

where $\phi_0$, $\Omega_0$, and $S_0$ relate to the external system, and $\phi$, $\Omega$, and S t the induced currents on S.

18. This gives at all points within S

$$\frac{d(F_0 + F)}{dt} = \frac{d(\psi_0 + \psi)}{dx}, \text{ \&c.},$$

where $\psi_0$ is the companion function to $\frac{dF_0}{dt}$, $\frac{dG_0}{dt}$, and $\frac{dH_0}{dt}$, and $\psi$ to $\frac{dF}{dt}$, $\frac{dG}{dt}$ and $\frac{dH}{dt}$.

19. If therefore $-dF/dt$, &c., are to be regarded as components of an electromotive force, notwithstanding their derivation from a potential within S, they will produce on S a distribution of free electricity having potential $-(\psi_0+\psi)$, and forming a complete *electric screen.*

20. There is no energy of mutual action between the electrostatic system, if it exists, and the electric currents, because

$$\iint\left(u\frac{d\psi}{dx}+v\frac{d\psi}{dy}+w\frac{d\psi}{dz}\right)dS = 0.$$

21 and 22. The effect of resistance generally.

23. Definition of self-inductive current shells, viz., those for which the values at any time, $t$, of the component currents, $u$, $v$, $w$, &c., are found from their values at a given epoch by multiplying by $\epsilon^{-\lambda t}$ where $\lambda$ is constant.

24. Investigation of the condition which $\phi$, the current function, must satisfy in order that a current shell may be capable of being made self-inductive.

25. If this condition be satisfied, the thickness of the shell which makes it self-inductive is determinate, the material being supposed uniform.

26. And $\lambda$ varies inversely as the thickness.

27. General property of self-inductive shells in presence of a corresponding magnetic field whose potential is $\Omega_0$ expressed by the equation—

$$\frac{d\Omega_0}{dt}+\frac{d\Omega}{dt}+\lambda\Omega = 0,$$

at all points within the shell, or on the opposite side of it to the inducing system.

28. Example (1), when $d\Omega/dt =$ constant.

29. Example (2), when $\Omega_0 = A\cos\lambda t$ and $\lambda$ constant.

30. Some consequences deduced from the last example.

Examples of self-inductive shells, viz.:—

31. Spherical shell.

32. Solid of revolution about the axis of $z$, $\phi$ being a function of $z$ only.

33. Any surface if $\phi$ be a function of $z$ only and $\psi$ a function of $x$ and $y$ only.

34. Example, an ellipsoidal shell.

35. Case of an infinite plane sheet as made self-inductive in certain cases.

36. Case of an infinite plane sheet when not self-inductive. Arago's disk.

37—40. *Self-inductive shells* bounded by a surface, S, when S is a *homogeneous function of $x$, $y$, and $z$.*

A solid formed of such shells and the action of outer shells upo inner ones, or *vice versâ.*

40. Case of a solid shell of small finite thickness.

41. Of statical distribution of electricity on a conductor as pro duced by variation of magnetic field.

42. Of non-self-inductive systems.

## II. "On the Relations of the Diurnal Barometric Maxima t certain critical Conditions of Temperature, Cloud, an Rainfall." By Henry F. Blanford, F.R.S. Receive March 30, 1888.

(Abstract.)

The author refers to an observation of Lamont's that the diurn barometric variation appears to be compounded of two distinct el ments, viz., a wave of diurnal period, which is very variable in d ferent places, and which appears to depend on the horizontal a vertical movements of the atmosphere and changes in the distrib tion of its mass, and a semi-diurnal element which is remarkal constant and seems to depend more immediately on the action of t sun. Then, referring to the theory of the semi-diurnal variati originally put forward by Espy, and subsequently by Davies a Kreil, the author points out that the morning maximum of pressu approximately coincides with the instant when the temperature rising most rapidly. This is almost exactly true at Prague, Yarka both in winter and summer, and in the winter months at Melbour At the tropical stations, Bombay, Calcutta, and Batavia, and at M bourne in the summer, the barometric maximum follows the inst of most rapid heating by a shorter or longer interval; and the autl remarks that this may probably be attributed to the action of conv tion, which must accelerate the time of most rapid heating near t ground surface; while the barometric effect, if real, must be det mined by the condition of the atmosphere up to a great height. W reference to Lamont's demonstration of the failure of Espy's theo a condition is pointed out which alters the data of the problem, v the resistance that must be offered to the passage of the pressu wave through the extremely cold and highly attenuated atmosphe strata, whose existence is proved by the phenomena of lumin meteors.

With respect to the evening maximum of pressure, it is pointed that very generally, and especially in India, and also at Melbour *there is a strongly-marked* minimum in the diurnal variation of cl *between sunset* and midnight, which, on an average, as at Allaha

and Melbourne, coincides with the evening maximum of the barometer. A similar coincident minimum, even more strongly marked, characterises the diurnal variation of the rainfall at Calcutta and Batavia in their respective rainy seasons. In the author's opinion these facts seem to point to a compression and dynamic heating of the cloud-forming strata, and he points to the existence of a small irregularity in the diurnal temperature curves of Prague, Calcutta, and Batavia, which may possibly be due to such action. It is further remarked that the evening maximum about coincides with the time when the evening fall of temperature, after a rapid reduction between 6 or 7 and 10 P.M., becomes nearly uniform in rate, and it is suggested that the former may possibly be determined by the check of the rate of collapse of the cooling atmosphere. But it is observed that both the morning and evening waves of pressure probably involve other elements than the forced waves, and are in part rhythmic repetitions of previous waves.

## III. "Effect of Chlorine on the Electromotive Force of a Voltaic Couple." By G. GORE, F.R.S. Received April 7, 1888.

If the electromotive force of a small voltaic couple of unamalgamated magnesium and platinum in distilled water, is balanced through the coil of a moderately sensitive galvanometer of about 100 ohms resistance, by means of that of a small Daniell's cell plus that of a sufficient number of couples of iron and German silver of a suitable thermoelectric pile (see 'Proceedings of the Birmingham Philosophical Society,' vol. 4, p. 130), the degree of potential being noted; and sufficiently minute quantities of very dilute chlorine-water are then added in succession to the distilled water, the degree of electromotive force of the couple is not affected until a certain definite proportion of chlorine has been added; the potential then suddenly commences to increase, and continues to do so with each further addition within a certain limit. Instead of making the experiment by adding chlorine-water, it may be made by gradually diluting a very weak aqueous solution of chlorine.

The minimum proportion of chlorine necessary to cause this sudden change of electromotive force is extremely small; in my experiments it has been 1 part in 17,000 million parts of water,* or less than a 7000th part of that required to yield a barely perceptible opacity in ten times the bulk of a solution of sal-ammoniac by means of nitrate of silver. The quantity of liquid necessary for acting upon the couple

* As 1 part of chlorine in 17,612 million parts of water had no visible effect, and 1 in *17,000 millions had a distinct effect*, the influence of the difference, or of 1 part *in 500,000 millions, has been detected.*

is small, and it would be easy to detect the effect of the above proportion, or of less than one ten-thousand-millionth of a grain of chlorine in one-tenth of a cubic centimetre of distilled water by this process. The same kind of action occurs with other electrolytes, but requires larger proportions of dissolved substance.

As the degree of sensitiveness of the method appears extreme, I add the following remarks :—The original solution of washed chlorine in distilled water was prepared in a dark place by the usual method from hydrochloric acid and manganic oxide, and was kept in an opaque, well-stoppered bottle in the dark. The strength of this liquid was found by means of volumetric analysis with a standard solution of argentic nitrate in the usual manner, the accuracy of the silver solution being proved by means of a known weight of pure chloride of sodium. The chlorine liquid contained 2·3 milligrammes, or 0·03565 grain of chlorine per cubic centimetre, and was just about three-fourths saturated.

One-tenth of a cubic centimetre of this solution ("No. 1"), or 0·003565 grain of chlorine was added to 9·9 c.c. of distilled water and mixed. One cubic centimetre of this second liquid ("No. 2"), or 0·0003565 grain of chlorine was added to 99 c.c. of water and mixed; the resulting liquid ("No. 3") contained 0·000003565 grain of chlorine per cubic centimetre. To make the solution ("No. 4") for exciting the voltaic couple, successive portions of one-tenth or one-twentieth cubic centimetre of "No. 3" liquid were added to 900 c.c. of distilled water and mixed.

I have employed the foregoing method for examining the states and degrees of combination of substances dissolved in electrolytes, and am also investigating its various relations.

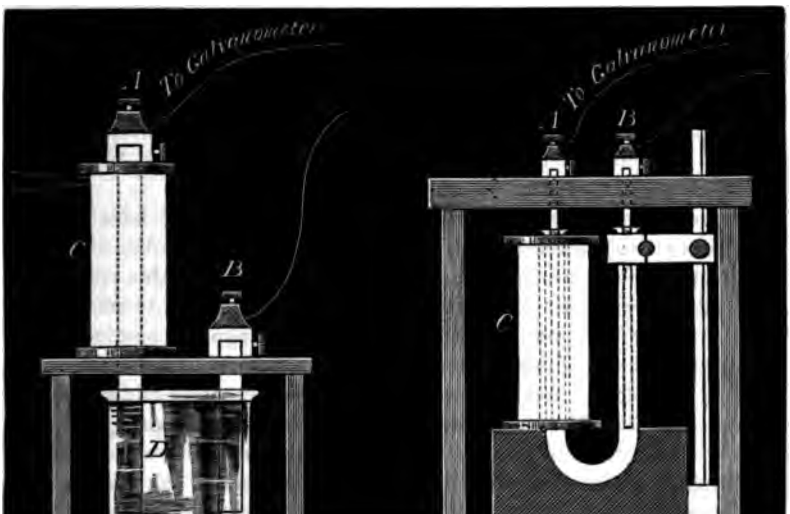
## IV. "Electro-chemical Effects on Magnetising Iron. Part II."* By Thomas Andrews, F.R.S.E., F.C.S. Communicated by Professor G. G. Stokes, P.R.S. Received April 9, 1888.

The novel electro-chemical effects observed between a magnetised and an unmagnetised bar when in circuit in certain solutions, recorded in the first part of this research, were of such an interesting character that I thought it desirable to extend the investigation. The present memoir contains the results of a further study of these magneto-chemical phenomena, which were found to be more marked and characteristic when experimenting with some of the reagents herein described. The method of experimentation was generally similar to that pursued and described in Part I, though it was necessary to

* For first part see vol. 42, p. 459.

introduce numerous modifications of detail and also some entirely new modes of experimentation hereafter referred to. Fig. 3 shows the form of apparatus, coil of 750 wraps, &c., used with the larger iron and steel bars. In this apparatus the ends of each pair of bars were deeply immersed in the solution contained in the vessel D, below the coil; liability to possible temperature errors from any heating of the coil was thus obviated. The unmagnetised bar B was made shorter than the bar A in the coil, so as to avoid partial magnetisation from outside induction of the coil, which would have been more liable to occur had the bar stood in full length parallel with the coil. This arrangement was found preferable when using large steel bars, as induced magnetism to any considerable extent of the bar B would have detracted from the full effect. In some instances, however, this precaution was not adopted. The apparatus, coil, &c., used with the smaller iron and steel bars is shown in fig. 4. A single-cell bichromate battery was employed in connexion with the coil for magnetisation during all the experiments recorded in this memoir.

FIG. 3. FIG. 4.




Scale 2 inches = 1 foot.

The bars were of specially prepared wrought iron and cast steel; the smaller bars were $8\frac{1}{2}$ inches long, 0·261 inch diameter, and the larger bars were $\frac{3}{4}$-inch diameter, the longer one (A) was $10\frac{1}{2}$ inches long, *and the shorter one (B)* $5\frac{1}{4}$ inches long; all the rods were finely *polished. The general physical* properties of the metals are given in

Table B. The cast-steel bars were employed in some of the expe ments, because after magnetisation in the coil their subsequent fluence as permanent magnets of lower strength could be observed referred to in course of this memoir.

Table B.—Physical Properties of the Metals.

| Description. | Contraction of area at fracture per cent. | Extension per cent. | Breaking strai per square inc of original area. |
|---|---|---|---|
| Small iron bars (Wortley best scrap) | 23 | 2·5 | tons. 45·82 |
| Small cast-steel bars | 22 | 2·0 | 55·42 |
| Large iron bars (Wortley best scrap) | 28 | 24·0 | 24·46 |
| Large cast-steel bars | 24 | 20·0 | 45·81 |

The small iron and steel bars were drawn through a wortle, t large iron and steel bars were rolled rods.

The chemical reagents employed as electrolytes consisted of so tions of bromine, ferric chloride, and chlorine-water, ferrous sulpha ferric chloride, cupric chloride, cupric sulphate, cupric nitrate, cup acetate, cupric bromide, nickel chloride, hydrochloric acid, nitric ac and potassium chlorate.

In the experiments with the smaller rods a pair of bars in ea experiment were immersed in the solution in the U-tube, in circ also with a delicate galvanometer, and after a suitable time h elapsed in every case for normal galvanic equilibrium to obtain, t bar A in the coil was magnetised, and the magneto-chemical eff recorded. It was found to vary with the nature of the metal a solution employed, and also with the extent of the magnetic saturati of the metals. The strength of the magnetism was practically t same in many of the experiments, and it was generally observed th difference in the strength of the solutions affected the results. other experiments with a uniform strength of solution, but in whi the magnetism of the metals was varied or reduced, the magne chemical effect became proportionately altered. The possibility error from temperature causes arising from any slight internal he ing of the coil has been referred to and dealt with in Part I ('R Soc. Proc.,' vol. 42, pp. 462–3). The apparatus, fig. 3, used in so of the present experiments was also conducive to accuracy in t respect. Moreover, the early and extensive development of t magneto-chemical effect, noticed in most cases, especially in

experiments with bromine, $HNO_3$, and the copper salts, affords sufficient indication that the liability to error from temperature causes was slight. To demonstrate that the magneto-chemical effect was not in these observations due to variation of temperature consequent on possible heating of the coil, a further set of experiments (Table C, Cols. 10 and 11, Divisions II) was made with solutions of ferrous sulphate and also of ferric chloride in another form of apparatus, wherein the unmagnetised bar B was surrounded by a slightly higher temperature (about 5° to 10° F.) during the observations than the magnetised bar A. This was accomplished by surrounding the limb of the U-tube containing the bar B, during the observation, with a specially constructed water-bath containing water at a temperature of about 5° to 10° F. above the temperature of the solution in the coil tube A, the difference of temperature was ascertained by small thermometers respectively placed in limbs A and B of the U-tube, another thermometer being in the water-bath. The magnetised bar was, however, able to maintain its positive position, notwithstanding the higher temperature around the unmagnetised one. The present memoir contains the results of many repeated experiments, each record in the tables being the average of a considerable number of observations. In Parts I and II a total of about 592 iron and steel bars have been experimented upon, 346 bars being required for the experiments of Part II.

### *Explanation of Results on Table C.*

*Hydrochloric Acid, conc., sp. gr.* 1·16, *Col.* 1.—No perceptible electro-chemical effect was obtainable with this reagent.

*Bromine and Potassium Bromide, Col.* 2, *Divisions I, II, and III.*—It was found that pure bromine for various reasons was too powerful a reagent to use in these experiments; a strong solution was therefore prepared of the following composition. Bromine, 1066·4 grains, potassium bromide, 520 grains, and $3\frac{1}{2}$ ozs. of water. This solution was very energetic in its action on the metals, and considerable care was required in conducting the experiments therewith; but with due precautions admirable results were obtained. The magneto-chemical effect was very great with this reagent, the E.M.F. between the magnetised and unmagnetised bars sometimes reaching as high as one-twentieth of a volt. The highest E.M.F. appeared to be manifested at or near the time of the energetic effervescing attack on the metal; though the E.M.F. between the bars was always considerable from the earliest commencement of the magnetisation of bar A in the coil. Experiments were made on both wrought-iron and steel bars. The experiments, *Col. 3, Divisions* I and II, were made with a much *weaker bromine solution, though* of similar general composition.

## Table C.

(An explanation of the details of this table will be found at the end

| Time from commencement of magnetisation. | E.M.F. in volt, and electro-chemical position of magnetised bar com with the unmagnetised bar, the positive or negative position o former being respectively indicated by the signs + and —. | | | | | |
|---|---|---|---|---|---|---|
| | Column 1. | Column 2. | | | Column 3 | |
| | Hydrochloric acid, conc. | Bromine in potassium bromide. | | | Bromine in po bromide (w solution) | |
| | Iron bars. | Iron bars. | | Steel bars. | Iron bars. | Ste |
| | — | I. | II. | III. | I. | |
| seconds. | | | | | | |
| 0 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | ( |
| 15 | ,, (No perceptible electro-chemical effect with this reagent.) | +0·011 | +0·014 | +0·016 | +0·007 | |
| 30 | ,, | +0·019 | +0·019 | +0·025 | +0·007 | +( |
| 45 | ,, | +0·026 | +0·025 | +0·029 | +0·007 | |
| minutes. | | | | | | |
| 1 | ,, | +0·020 | +0·024 | +0·032 | +0·007 | +( |
| 2 | ,, | +0·024 | +0·031 | +0·033 | +0·008 | +( |
| 3 | ,, | +0·028 | +0·045 | +0·028 | +0·006 | +( |
| 4 | ,, | +0·025 | +0·058 | +0·025 | +0·009 | +( |
| 5 | ,, | +0·016 | +0·060 | +0·016 | +0·007 | +( |
| 6 | ,, | +0·022 | +0·046 | +0·012 | +0·006 | +( |
| 7 | ,, | +0·022 | +0·010 | +0·011 | +0·004 | +( |
| 8 | ,, | +0·023 | +0·009 | +0·011 | +0·004 | +( |
| 9 | ,, | +0·025 | +0·007 | +0·014 | +0·004 | |
| 10 | ,, | +0·026 | +0·011 | | +0·003 | |
| 11 | ,, | +0·023 | +0·012 | | +0·004 | |
| 12 | ,, | +0·028 | +0·012 | +0·011 | +0·004 | |
| 13 | ,, | +0·030 | +0·011 | +0·009 | +0·004 | |
| 14 | ,, | +0·022 | +0·012 | +0·011 | +0·009 | |
| 15 | ,, | +0·013 | +0·013 | +0·006 | +0·009 | |
| 16 | ,, | +0·004 | +0·013 | +0·004 | +0·010 | |
| 17 | ,, | +0·006 | +0·013 | +0·011 | +0·009 | |
| 18 | ,, | +0·009 | +0·009 | +0·011 | +0·011 | |
| 19 | ,, | +0·006 | +0·010 | +0·011 | +0·009 | |
| 20 | ,, | +0·006 | +0·008 | +0·014 | +0·010 | |
| 25 | ,, | +0·005 | +0·001 | +0·010 | +0·009 | |
| 30 | ,, | +0·002 | +0·002 | +0·009 | +0·004 | |

Table C—*continued.*

| E.M.F. in volt, and electro-chemical position of magnetised bar compared with the unmagnetised bar, the positive or negative position of the former being respectively indicated by the signs + and −. | | | | | | |
|---|---|---|---|---|---|---|
| Column 4. | | Column 5. | Column 6. | Column 7. | Column 8. | Column 9. |
| Ferric chloride and chlorine water. | | Nitric acid, 250 grs., and potassium chlorate solution, 500 grs. | Nitric acid, 200 grs., and water, 600 grs. | Nitric acid, 300 grs., and potassium chlorate solution, 1000 grs. | Nitric acid, 250 grs., and potassium chlorate solution, 1000 grs. | Nitric acid, 200 grs. and potassium chlorate solution, 1000 grs. |
| Iron bars. | Steel bars. | Iron bars. | Iron bars. | Iron bars. | Iron bars. | Iron bars. |
| I. | II. | — | — | — | — | — |
| 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 |
| −0·006 | −0·006 | | | | | |
| −0·007 | −0·009 | | | | | |
| −0·009 | −0·009 | | | | | |
| −0·009 | −0·007 | +0·011 | | +0·012 | +0·010 | +0·006 |
| −0·009 | −0·009 | +0·013 | +0·004 | +0·011 | +0·005 | +0·004 |
| −0·006 | −0·009 | +0·014 | | +0·010 | +0·006 | +0·004 |
| −0·006 | −0·006 | +0·018 | +0·005 | +0·008 | | +0·003 |
| −0·006 | −0·008 | +0·027 | +0·010 | +0·009 | +0·010 | +0·004 |
| −0·006 | −0·007 | +0·018 | +0·009 | | | |
| −0·009 | −0·006 | +0·014 | +0·011 | +0·007 | +0·007 | +0·006 |
| −0·007 | −0·006 | | | | | |
| −0·009 | −0·005 | | +0·014 | | +0·006 | |
| −0·007 | −0·004 | | +0·013 | +0·010 | +0·007 | +0·005 |
| −0·007 | −0·004 | | +0·010 | +0·011 | +0·009 | |
| −0·005 | −0·003 | | +0·006 | +0·001 | +0·009 | +0·005 |
| −0·004 | −0·003 | | +0·005 | | +0·009 | +0·004 |
| −0·002 | −0·003 | | +0·004 | | +0·008 | +0·004 |
| −0·002 | −0·003 | | +0·006 | | +0·006 | +0·003 |
| | Magnetisation here ceased, the after effect was due to residual magnetism of the steel. | | +0·006 | | +0·009 | |
| | | | +0·007 | | +0·009 | |
| | | | +0·009 | | +0·007 | |
| | | | +0·008 | | | |
| | | | +0·008 | | | |
| | +0·003 | | +0·007 | | | |
| | +0·003 | | | | | |
| | +0·008 | | | | | |
| | +0·008 | | | | | |
| | +0·006 | | | | | |
| | +0·002 | | | | | |
| | +0·006 | | | | | |
| | +0·003 | | | | | |
| | +0·002 | | | | | |
| | +0·002 | | | | | |
| | +0·002 | | | | | |

Table C—*continued.*

| Time from commencement of magnetisation. | E.M.F. in volt, and electro-chemical position of magnetised bar compar the unmagnetised bar, the positive or negative position of the forme respectively indicated by the signs + and −. | | | | | | |
|---|---|---|---|---|---|---|---|
| | Column 10. | | | | Column 11. | | |
| | Ferrous sulphate. | | | | Ferric chloride. | | |
| | Iron bars. | | Steel bars. | | Iron bars. | | S |
| | I. | II. | III. | IV. | I. | II. | |
| seconds. | | | | | | | |
| 0 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | |
| 30 | | | 0·000 | 0·000 | | | |
| minutes. | | | | | | | |
| 1 | | | 0·000 | 0·000 | | | |
| 2 | | | +0·0004 | 0·000 | | | |
| 3 | | | | | | | |
| 4 | | | | | | | |
| 5 | +0·001 | 0·000 | +0·001 | 0·000 | 0·000 | | |
| 6 | | | | | | | |
| 7 | | | | | | | |
| 8 | | | | | | | |
| 9 | | | | | | | |
| 10 | +0·001 | 0·000 | +0·001 | +0·0004 | +0·0004 | +0·002 | |
| 15 | +0·002 | −0·004 | +0·001 | +0·001 | +0·001 | +0·001 | |
| 20 | +0·003 | 0·000 | +0·001 | +0·001 | +0·001 | +0·001 | |
| 25 | +0·003 | +0·001 | +0·001 | +0·001 | +0·001 | +0·001 | |
| 30 | +0·003 | +0·001 | +0·002 | +0·001 | +0·002 | +0·001 | |
| 35 | +0·004 | +0·001 | +0·002 | +0·001 | +0·002 | +0·001 | |
| 40 | +0·004 | +0·001 | +0·002 | +0·001 | +0·002 | +0·001 | |
| 45 | +0·004 | +0·001 | +0·002 | +0·001 | +0·002 | +0·001 | |
| 50 | +0·004 | +0·001 | +0·002 | +0·001 | +0·002 | +0·001 | |
| 55 | +0·004 | +0·001 | +0·002 | +0·001 | +0·002 | +0·002 | |
| hours. | | | | | | | |
| 1 | +0·004 | +0·001 | +0·003 | +0·001 | +0·001 | +0·002 | |
| 1½ | | +0·002 | | | | +0·001 | |
| 2 | | +0·002 | +0·007 | +0·002 | | +0·001 | |
| 2½ | | | | | | | |
| 3 | | | | +0·002 | | | |
| 5 | | | | | | | |
| 6 | | | +0·003 | +0·001 | | | |
| 9 | | | +0·005 | | | | |
| 12 | | | | | | | |
| 16 | | | | | | | |
| 20 | | | | +0·001 | | | |
| 24 | | | | | | | |
| 36 | | | | | | | |
| 40 | | | | | | | |
| 44 | | | | +0·006 | | | |

All the steel bars on the above table were magnetised in the coil for a short time, not *ten minutes in each case*, so that the effects subsequent to this were due only to tl *magnetism of the steel;* thus there would be no liability to aberration from temperatur

Table C—*continued.*

| me from mmencement of gnetisation. | E.M.F. in volt, and electro-chemical position of magnetised bar compared with the unmagnetised bar, the positive or negative position of the former being respectively indicated by the signs + and −. | | | | | | |
|---|---|---|---|---|---|---|---|
| | Column 12. | | Column 13. | | Column 14. | Column 15. | |
| | Cupric chloride. | | Cupric sulphate. | | Cupric nitrate. | Cupric acetate. | |
| | Iron bars. | Steel bars. | Iron bars. | Steel bars. | Iron bars. | Iron bars. | Steel bars. |
| | I. | II. | I. | II. | — | I. | II. |
| **conds.** | | | | | | | |
| **0** | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 |
| **15** | +0·011 | +0·011 | +0·018 | +0·014 | | | |
| **30** | +0·015 | +0·013 | +0·022 | +0·015 | | | |
| **45** | +0·020 | +0·016 | +0·026 | +0·019 | +0·001 | | |
| **inutes.** | | | | | | | |
| **1** | +0·027 | +0·022 | +0·029 | +0·024 | +0·001 | +0·001 | +0·0004 |
| **2** | +0·038 | +0·029 | +0·035 | +0·029 | +0·002 | +0·001 | +0·001 |
| **3** | +0·047 | +0·034 | +0·040 | +0·028 | +0·002 | +0·002 | +0·001 |
| **4** | +0·045 | +0·038 | +0·043 | +0·028 | +0·003 | +0·003 | +0·001 |
| **5** | +0·042 | +0·039 | +0·050 | +0·040 | +0·004 | +0·004 | +0·001 |
| **6** | +0·036 | +0·039 | +0·060 | +0·044 | +0·004 | +0·004 | +0·001 |
| **7** | +0·027 | +0·038 | +0·063 | +0·049 | +0·004 | +0·004 | +0·001 |
| **8** | +0·025 | +0·040 | +0·064 | +0·054 | +0·004 | +0·004 | +0·002 |
| **9** | +0·026 | +0·029 | +0·069 | +0·063 | +0·004 | +0·005 | +0·002 |
| **10** | +0·023 | +0·030 | +0·072 | +0·060 | +0·005 | +0·005 | +0·002 |
| **12½** | +0·020 | +0·033 | +0·087 | +0·081 | +0·005 | +0·006 | +0·002 |
| **15** | +0·020 | +0·039 | +0·107 | +0·083 | +0·005 | +0·008 | +0·003 |
| **17½** | +0·023 | +0·040 | +0·123 | +0·081 | +0·008 | +0·009 | +0·003 |
| **20** | +0·025 | +0·040 | +0·104 | +0·083 | +0·009 | +0·010 | +0·003 |
| **25** | +0·024 | +0·051 | +0·114 | +0·072 | +0·013 | +0·011 | +0·004 |
| **30** | +0·019 | +0·056 | +0·113 | +0·098 | +0·013 | +0·014 | +0·004 |
| **35** | +0·015 | +0·055 | +0·094 | +0·072 | +0·014 | +0·019 | +0·004 |
| **40** | +0·017 | +0·047 | +0·103 | +0·067 | +0·015 | +0·022 | +0·004 |
| **45** | +0·014 | +0·028 | +0·092 | +0·069 | +0·017 | +0·015 | +0·004 |
| **hours.** | | | | | | | |
| **1** | | +0·020 | | | +0·019 | | +0·003 |
| **1¼** | | | | | +0·014 | | |
| **1½** | | +0·026 | | | | | +0·005 |
| **2** | | | | | | | +0·007 |
| **2½** | | +0·022 | | | | | +0·009 |
| **3** | | | | | | | +0·009 |
| **3½** | | | | | | | +0·007 |
| **4** | | +0·054 | | | | | |

In the experiments with the steel bars in cupric acetate, Col. 15, Division II, magnetisation of the bar A ceased at 45 minutes, *the subsequent* results being due to the residual magnetism.

Table C—*continued.*

| Time from commencement of magnetisation. | E.M.F. in volt, and electro-chemical position of magnetised bar co[m…] with the unmagnetised bar, the positive or negative position [of the] former being respectively indicated by the signs + and −. | | | | | |
|---|---|---|---|---|---|---|
| | Column 16. | | | | Column 1[7.] | |
| | Cupric bromide. | | | | Nickel chlo[ride.] | |
| | Iron bars. | Steel bars. | Iron bars. | Steel bars. | Iron bars. | St[eel bars.] |
| | I. | II. | III. | IV. | I. | |
| seconds. | | | | | | |
| 0 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | |
| 15 | +0·013 | +0·007 | 0·000 | +0·009 | | |
| 30 | +0·019 | +0·011 | +0·002 | +0·011 | | |
| 45 | +0·022 | +0·012 | +0·004 | +0·013 | | |
| minutes. | | | | | | |
| 1 | +0·025 | +0·014 | +0·005 | +0·016 | | |
| 2 | +0·029 | +0·018 | +0·009 | +0·027 | | |
| 3 | +0·031 | +0·024 | +0·011 | +0·030 | | |
| 4 | +0·033 | +0·028 | +0·013 | +0·034 | | |
| 5 | +0·034 | +0·031 | +0·016 | +0·038 | | |
| 6 | +0·038 | +0·031 | +0·018 | +0·040 | | |
| 7 | +0·029 | +0·035 | +0·023 | +0·043 | | |
| 8 | +0·024 | +0·037 | +0·025 | +0·044 | | |
| 9 | +0·024 | +0·040 | +0·025 | +0·048 | | |
| 10 | +0·024 | +0·036 | +0·023 | +0·050* | | |
| 12½ | +0·030 | +0·039 | +0·020 | +0·051 | | |
| 15 | +0·041 | +0·035 | +0·023 | +0·056 | +0·001 | |
| 17½ | +0·041 | +0·039 | | +0·059 | | |
| 20 | +0·024 | +0·039 | +0·023 | +0·063 | +0·001 | |
| 25 | +0·014 | +0·034 | +0·025 | +0·068 | +0·002 | |
| 30 | +0·019 | +0·040 | +0·025 | +0·072 | +0·002 | |
| 35 | +0·021 | +0·031 | +0·025 | +0·075 | +0·003 | |
| 40 | +0·049 | +0·027 | +0·027 | +0·077 | | |
| hours. 45 | +0·023 | +0·006 | +0·027 | +0·077 | | |
| 1 0 | | | +0·023 | +0·077 | +0·004 | |
| 2 0 | | | | +0·064 | | |
| 3 0 | | | | +0·058 | | |
| 5 0 | | | | +0·048 | | |
| 7 0 | | | | +0·046 | | |
| 20 0 | | | | +0·010 | | |
| 24 0 | | | | +0·002 | | |
| 26 0 | | | | 0·000 | | |

* Column 16, Division IV, steel bars in cupric bromide, magnetisation here ceased, [the] effect was due to residual magnetism of the steel.

† Column 17, Division II, steel bars in nickel chloride, magnetisation here ceased, [the] *effect was due to residual* magnetism of the steel.

*Ferric Chloride and Chlorine Water, Col.* 4, *Divisions I and II.*—This solution consisted of 250 fluid grains of a concentrated solution of $Fe_2Cl_6$ and 750 fluid grains of saturated chlorine water. In these experiments the electro-negative position assumed by the magnetised bar formed an exception to the general rule, which, I think, may probably to some extent be explained on the supposition of the diamagnetic properties of the dissolved chlorine; the magnetised bar being somewhat less attacked by the free chlorine than the unmagnetised rod. When the chlorine had exhausted its action on the metal, the electro-chemical reaction became gradually reversed, and the magnetised bar then assumed the electro-positive position (see Col. 4, Division II), as in the case of normal ferric chloride solution only. To show that the above negative effect was due only to magnetic influence, various experiments with $Fe_2Cl_6$ and chlorine water were made, in which it was found that on ceasing to magnetise the bar A for a few moments, the E.M.F. decreased, and the magnetised bar A assumed a less negative position, but on again connecting the battery to the coil, the magnetised bar therein assumed a more electro-negative position.

*Nitric Acid, sp. gr.* 1·42, *and Potassium Chlorate, Cols.* 5, 7, 8, *and* 9.—These experiments made with apparatus, fig. 4, with solutions containing varied proportions of $HNO_3$ and $K_2ClO_3$, are confirmatory of the results obtained in Part I, and also indicate that these magneto-chemical effects are greater in stronger solutions. On ceasing to magnetise the bar A, in course of any of these experiments, the needle of the galvanometer fell to zero, and on remagnetising the bar A its electro-positive position was re-asserted.

*Ferrous Sulphate, Col.* 10, *Divisions I, II, III, and IV*, a saturated solution of the salt.

*Division I.*—This set of experiments was conducted on the large polished wrought-iron bars, $\frac{3}{4}$-inch diameter, with apparatus fig. 3, the magnetisation of bar A being continuous to the end of each observation.

*Division II.*—These experiments were made with small iron bars in apparatus, fig. 4, the solution containing the unmagnetised bar being maintained at a temperature of about 5° to 10° F. above the temperature of the solution in which the magnetised bar was immersed.

*Division III.*—In these observations large steel bars $\frac{3}{4}$-inch diameter were employed in the arrangement of apparatus delineated in fig. 3. The bar A in the coil was magnetised for a few minutes only at the commencement, and, as the metal was steel, it retained a permanent residual magnetism, which was allowed to complete the result. The magneto-chemical effect was not so great in these instances, owing to the magnetism of the bar being less than when the action of the powerful coil was prolonged thereon, as in the other experiments

This class of observations indicated that the results were influenced by the extent to which the metal was magnetised; the latter fact was more distinctly shown in course of experiments with bromine and the salts of copper.

*Division IV.*—These experiments were made on the small steel bars with apparatus, fig. 4; the general results were similar to those obtained with the larger bars, though somewhat less in extent. The bar A in the coil was magnetised for a short time only at the commencement, and the induced permanent magnetism allowed to complete the result.

*Ferric Chloride, Col.* 11, *Divisions I, II and III,* was a saturated solution of the salt in water. The experiments in Division I were made in apparatus, fig. 4, equality of temperature obtaining between the two limbs of the U-tube. The observations of Division II were made in the water-bath apparatus previously alluded to, with a difference of temperature of about 5° to 10° F. in favour of the unmagnetised bar; the magnetic influence was, however, sufficient to overcome this temperature obstacle, and even under such conditions the magnetised bar maintained its electro-chemical position, though not to the full extent. On ceasing to magnetise a bar in the above reagent, the E.M.F. steadily diminished, and on again applying magnetisation the magnetised bar resumed its positivity. The observations in Division III were made on pairs of the small steel bars under equal temperature conditions. At the end of forty hours there was a perceptibly greater deposit of flocculent oxide of iron in the tube containing the magnetised bar.

*Cupric Chloride, Col.* 12, *Divisions I and II,* consisted of a concentrated solution of the salt in water, such as is usually employed in dissolving out the metallic iron in the carbon determination of iron analyses. The magneto-chemical effect with this reagent was of considerable magnitude, a powerful effect commencing from first magnetisation of the bar A, and largely though steadily increasing. On ceasing to magnetise the bar A the galvanometer deflections were reduced; but on again bringing the magnetising coil into action, the magnetised bar A steadily re-asserted its strong positive position in course of a few moments. These magnetic effects were not of such a nature as to produce a very violent fling of the galvanometer, but manifested a steady and permanent character, though in most instances deflections commenced at once on magnetising the bar A, and afterwards continued steadily to increase till the maximum point was reached. On the completion of an experiment, both bars were of course covered with electro-deposited metallic copper; but in many instances the colour of the solution in the limb of the U-tube which had contained the magnetised bar, was of a rather lighter tint, showing that a somewhat greater deposition of copper had occurred therein. The experi-

ments in Division I were with pairs of the small wrought-iron bars, and the observations in Division II were made on pairs of the small steel bars. It will be noticed that the E.M.F. was greater in the case of the wrought-iron than with the steel bars.

*Cupric Sulphate, Col.* 13, *Divisions I and II*, a concentrated solution of the salt in water. The remarks made on the magneto-chemical effects with cupric chloride apply generally to the reactions obtained with cupric sulphate; it will be noticed, however, that the effect was more extensive when employing the latter salt.

*Cupric Nitrate, Col.* 14, was composed of a saturated solution of the salt in water. The magneto-chemical effect was observed with this reagent, though it was more limited in extent than when using either CuCl, CuBr, or $CuSO_4$.

*Cupric Acetate, Col.* 15, *Divisions I and II.*—This was a concentrated solution of the salt in which the effect was small; but it was distinctly noticeable.

*Cupric Bromide, Col.* 16, consisted of a saturated solution of the salt in water. Highly interesting and very marked results were noticed in the experiments with this reagent. The observations in Divisions I and II were made with small rods of wrought iron and steel in apparatus, fig. 4, the results recorded in Divisions III and IV being obtained with large iron and steel bars $\frac{3}{4}$-inch diameter, and using apparatus, fig. 3. A weaker solution of cupric bromide was employed for the iron bar experiments in Division III, and the bars were not immersed so deeply in the solution. The electro-positive position of the coil-bar A was dependent on the extent of its magnetisation, in these as in the other experiments, and the effects with cupric bromide were generally similar to those obtained with cupric chloride.

*Nickel Chloride, Col.* 17, *Divisions I and II*, was a concentrated solution of the salt in water.

*Sulphate of Iron.*—A pair of steel bars were left in a yellow oxidised solution of sulphate of iron in apparatus, fig. 4, for twenty-four hours, the bar A having been magnetised for a short time at commencement only, the residual magnetism being allowed to complete the effect; an E.M.F. of 0·011 volt was gradually reached, the magnet bar being in the positive position.

### *The Electro-chemical Effect as between the Magnetic Polar Terminals and Equator.*

In casting about for an explanation of these magneto-chemical phenomena, it seemed probable that the effect might possibly be connected with the local currents which are shown below to develop in a magnetised bar between the more highly and less magnetised parts thereof, when the rod was immersed in suitable solutions acting

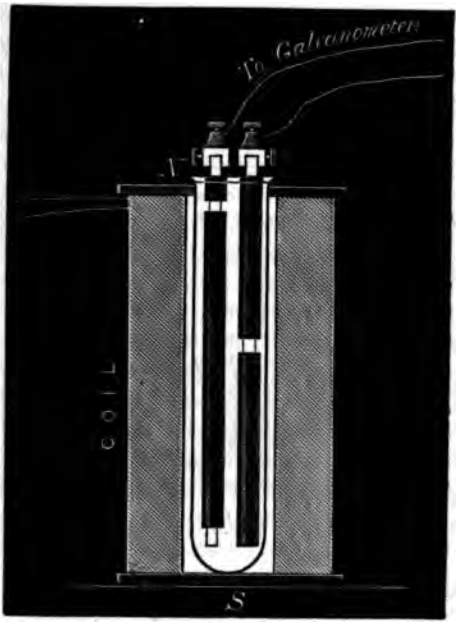
chemically upon it. I therefore made the following experiments which demonstrate the interesting fact, that when a magnetised bar is thus immersed a local galvanic current is set up between the polar terminals and central equator or less magnetised portion of the bar, the more magnetically-neutral zones acting electro-negatively in relation to the poles. Under these induced conditions it becomes obvious that a magnetised bar, forming one element of a galvanic couple, would be more acted upon than an unmagnetised one, forming the other element in the same solution. Hence, one explanation is afforded of the electro-chemical positive position generally manifested by the magnetised bar in course of the research. The experimental demonstration of these local currents in a magnetised rod was conducted as follows:—

A pair of polished soft-iron bars, $6\frac{1}{4}$ inches long, 0·261 inch diameter, cut adjacently from a larger rod, were each covered with black india-rubber tubing, a small portion, one quarter of an inch at each end of one bar (the flat disk at the end being coated with black varnish) and half an inch in the centre of the other rod, being the only portions exposed, and an equality of surface exposure being thus obtained. The two rods were placed in the tube containing the solution, and were connected in circuit with the galvanometer. The tube containing them was placed in the coil, and on magnetising the rods by means thereof, the rod whose polar terminals were exposed to the action of the solution became electro-positive to the other bar. Similar results were obtained when either a north pole or a south pole was exposed singly as one element in connexion with a central equator as the other. Many repeated experiments were made with apparatus shown on fig. 5, and about forty-six india-rubber-covered bars were used in this part of the investigation. The results are given in Table D (p. 166).

*Nitric Acid and Potassium Bichromate, Col.* 3.—On ceasing to magnetise the bar A in course of any experiment the galvanometer deflections almost immediately fell to zero, and on again magnetising the bar A deflections went up, the polar terminals resuming their positive position. In this experiment the central equator had an exposed surface of $\frac{1}{2}$ inch and each polar terminal $\frac{1}{4}$ inch; another experiment was made in which the exposed part of the central equator was only $\frac{1}{4}$ inch and each polar terminal $\frac{1}{8}$ inch; the results were the same though of less extent. Similar results were obtained on ceasing at any time to magnetise the bars in the cupric chloride solutions, Cols. 1 and 2, though less extensive.

*Nitric Acid and Potassium Bichromate, Col.* 4.—On ceasing to magnetise at end of any experiment, the deflections of the galvanometer fell some degrees; but on re-magnetising, deflections rose again, S. pole being positive.

FIG. 5.




Section of interior of coil.

*'upric Chloride, Cols.* 5 *and* 6.—On ceasing to magnetise, galvano-er deflections fell some degrees, but rose again on re-magnetising.

'uring an investigation of the possible electro-chemical effect veen the polished end disks or polar terminals only, of straight id steel magnets, there were indications, under certain conditions when the magnets were immersed as elements in some electro-s, of a tendency on the part of the N. terminal plane of the net to become from some cause electro-positive to the S. terminal e, when the magnets were placed parallel some distance apart in pright position. The lower end of each magnet exposed in the tion was covered with black india-rubber tubing, so that the flat shed disks at the terminals only were exposed to the action of the rolyte. This apparent tendency seemed somewhat singular, and her experimentation is required before arriving at definite con-ons; it seemed desirable however to allude to this apparent ation.

hope to make other observations in this direction, and in course hese *to utilise some valuable* experimental suggestions which *ssor Stokes has kindly made.*

Table D.

| Time from commencement of magnetisation. | Current between polar terminals and central equator of magnetised iron bars whe tions, E.M.F. in volt, the central equator was electro-negative in every experim | | | | | |
|---|---|---|---|---|---|---|
| | Experiments with magnet centre and both poles exposed. | | | Experiments with magnet centre and south pole only exposed. | Experiments with magnet centre and north pole only exposed. | Ex wit ce sout ( |
| | Column 1. | Column 2. | Column 3. | Column 4. | Column 5. | C |
| | Cupric chloride solution. | Cupric bromide solution. | Nitric acid, sp. gr. 1·42, and potassium bichromate (concentrated solution) in equal volumes. | Nitric acid, sp. gr. 1·42, and potassium bichromate solution in equal volumes. | Cupric chloride solution. | |
| seconds. | | | | | | |
| 0 | 0·000 | 0·000 | 0·000 | 0·000 | 0·000 | |
| 15 | 0·007 | | | 0·002 | 0·014 | |
| 30 | 0·014 | 0·004 | | 0·004 | 0·023 | |
| 45 | 0·022 | | | | 0·027 | |
| minutes. | | | | | | |
| 1 | 0·015 | 0·005 | 0·004 | 0·006 | 0·030 | |
| 2 | 0·022 | 0·007 | 0·009 | 0·006 | 0·038 | |
| 3 | 0·025 | 0·009 | 0·014 | 0·006 | 0·051 | |
| 4 | 0·033 | 0·009 | 0·023 | 0·006 | 0·061 | |
| 5 | 0·039 | 0·009 | 0·023 | 0·007 | 0·063 | |
| 6 | 0·040 | | | | 0·064 | |
| 7 | 0·044 | | 0·014 | 0·007 | 0·064 | |
| 8 | 0·050 | | | | 0·061 | |
| 9 | 0·044 | | | | 0·061 | |
| 10 | 0·042 | 0·013 | 0·009 | 0·006 | 0·061 | |
| 12½ | 0·038 | | | 0·005 | 0·054 | |
| 15 | 0·038 | 0·009 | 0·009 | 0·004 | 0·051 | |
| 17½ | 0·036 | 0·014 | | | | |
| 20 | 0·037 | 0·015 | 0·009 | 0·004 | 0·053 | |
| 25 | 0·032 | 0·017 | 0·006 | 0·004 | 0·053 | |
| 30 | 0·036 | 0·020 | 0·004 | | 0·051 | |
| 35 | 0·034 | 0·020 | | | 0·051 | |
| 40 | 0·031 | 0·028 | | | 0·051 | |
| 45 | 0·028 | 0·029 | | | 0·051 | |
| hour. | | | | | | |
| 1 0 | | 0·034 | | | | |
| 1 30 | | 0·043 | | | | |
| 2 30 | | 0·058 | | | | |

*The Electro-chemical Effect in Relation to the Passive State of Iron.*

Soon after commencing Part I of this research I conceived that the passive state of iron in strong nitric acid would either to some extent be affected, or perhaps overcome, by magnetic influences of a similar nature to those in the experiments on which I am engaged. Preliminary experiments were made and interesting results obtained in connexion with the influence of magnetisation on the action of strong nitric acid on iron and steel. I have obtained, under certain conditions, currents flowing from a magnetised bar to an unmagnetised one in strong nitric acid (sp. gr. 1·42). The currents representing an E.M.F. varying, according to circumstances, from about 0·011 volt and upwards. After considerable experimentation I feel convinced that induced local currents of the nature of those shown above in Table D were instrumental in causing the magnetised bars to be more acted upon than the unmagnetised ones in the strong nitric acid (sp. gr. 1·42), and such currents are essential in reducing the passivity of iron in nitric acid.

I hope to have further communications to make with respect to this interesting part of my research.

In Parts I and II of this research, the results of a quantitative study of these magneto-chemical phenomena have been recorded, the effect in connexion with a considerable variety of typical reagents having been carefully observed. With some reagents the effect was found to be comparatively small, in other instances it was very considerable, as in the case of bromine, many of the salts of copper, nitric acid, and similar strong corrosive solutions. The result was dependent both on the strength of the solution and on the extent of the magnetisation of the metal. In most cases with powerfully oxidising reagents the effect was of an electro-positive nature, but in a few other instances (such as $H_2SO_4$, HCl dil., $Fe_2Cl_3$ with chlorine) the reaction partook of a negative character in relation to the electro-chemical position of the magnetised bar. It is not easy to account for these variations in the nature of the effect; I think, however, it may be surmised that in these exceptional instances the results were possibly to some extent influenced by the diamagnetic properties of some of the solutions, or of the gases evolved therein. In some of the compound solutions, such as $Fe_2Cl_3$ and chlorine water, a species of magnetic selection apparently occurred. In the experiments with $Fe_2Cl_3$ solution without chlorine, the magnetised bar was electro-positive; but when using this reagent combined with chlorine water (see Table C, Col. 4, Divisions I and II), the magnetised bar became electro-negative, possibly owing to the diamagnetic property of *the free chlorine influencing its* action on the magnetised bar. *When, however, the free chlorine* had exhausted its direct action on

the metal, there remained only a solution of $Fe_2Cl_3$, in which the magnetised bar A gradually assumed its normal electro-positive position; this reaction is exemplified by the results in Table C, Col. 4, Division II. The comparative non-activity of HCl on magnetised bars is very singular, and at present unaccountable. In conclusion, I may state that this research has shown that a current flows from a magnetised bar towards an unmagnetised one, when the two are immersed in suitable solutions, and that the result was dependent both on the nature and strength of the solution, and also on the extent of the magnetisation of the metal. It has also been demonstrated that when a magnetised rod constitutes one element in a suitable electrolyte acting upon it, local currents flow from the more highly magnetised polar terminals towards the less magnetised or neutral equatorial portions. These conditions would cause the magnetised rod to be more generally acted upon by the electrolyte, the composition of the solution surrounding it being thereby also affected, and to a considerable extent this might account for its electro-positive position compared with the unmagnetised rod, otherwise under the same conditions. Observations have also been made on the influence of magnetisation in relation to the passive state of iron in nitric acid, with interesting results. In the present state of the inquiry it is preferable to confine oneself to a simple record of facts; I think, however, it has been clearly demonstrated in course of the numerous and varied experiments of this research, that the magnetisation of iron and steel influences the action of reagents upon the metal.

## V. "Report on the Capacities, in respect of Light and Photographic Action, of two Silver on Glass Mirrors of different Focal Lengths." By the Rev. C. Pritchard, D.D., F.R.S., Savilian Professor of Astronomy, Oxford. Received April 18, 1888.

In May of last year, I was requested by a Committee on stellar photography, appointed by the Council of the Royal Society, to examine the comparative photographic capacities of two silver on glass mirrors, each having an aperture of 15 inches, but of different focal lengths, viz., 80 inches and 120 inches respectively. In the present report these will be designated by the symbols $\frac{15}{80}$-inch and $\frac{15}{120}$-inch. The mirrors in question were provided by the generosity of Dr. Warren de la Rue. Various unforeseen difficulties incidental to pioneering in a science still in its infancy have intervened, unavoidably impeding the progress of the enquiry. The chief among these have been:—1. The comparatively imperfect automatic mechanism of the driving apparatus attached to the telescope carrying the

mirrors; 2. The difficulty of adjusting the camera or plate holder perpendicularly to the axis of the mirror, on a temporary mounting, and distant from the workshop of the optician; 3. An abnormal sky which has continually perplexed astronomers during many months.

It must not be overlooked, that even the considerable precision, necessary or desirable in the clock motion of a telescope used for micrometrical measures, is comparatively useless for astronomical photography; for in this latter case the momentary swerving of the telescope through even a second or two of arc, may be fatal to the circular form of the star images impressed on the plate; and, moreover, it is necessary to maintain this accuracy of steady motion, not merely for a very few minutes at a time, but occasionally for half an hour, or a full hour, or even more. It is true that resort may be had, and in fact must always be had, to the old method of supplementing the driving machine by the occasional assistance of eye and hand; but unless that machinery is approximately perfect, the strain upon the observer's attention becomes practically insupportable. This perfect steadiness of motion is also necessary from another point of view, because in its absence, it will not be easy to distinguish between the effects of unsteady motion and any optical defect of the mirror. Happily these difficulties have been at length overcome; and in the month of January last, by the aid of an improved screw, worked on a new engine by Sir H. Grubb, and a subsidiary electrical control connecting the driving apparatus with a seconds pendulum, I had the pleasant satisfaction of hearing from Mr. Jenkins, the assistant chiefly engaged in the present operation, that he now felt no severe strain or stress of attention in watching and occasionally aiding the motion, during the space of an hour or more on the rare occasions when the variability of the sky permitted such long exposures. I am not here speaking of my own experience alone, but I have reason to know that the same troubles have been shared to a greater or less extent by all the few eminent observers who are in this country employed in a similar pursuit. A modification of the ingenious contrivance by which the desired effects have been produced has been recently exhibited by Sir H. Grubb at the Royal Astronomical Society and at the Society of Arts in London.

The mirrors referred to above, were mounted in succession on the tube of the large equatorial in the Oxford University Observatory, and they proved to be of that excellent optical quality which might be expected in Mr. With's best performance.

The points to which I chiefly directed my attention in the examination of these mirrors were as follows:—

I. The general *character* of the stellar images impressed by the *two mirrors, absolute* and comparative.

II. The relative amounts of light reflected by each.

III. Their relative capacities in respect of distortion in the figure of the stellar images, and the optical distortion of the field.

IV. Their photographic capacity in respect of the faintest stars impressed on plates, with exposures of given duration.

### I. *The General Character of the Stellar Images impressed.*

It was originally proposed to employ the same sized plate, viz., 4 inches square, for both mirrors, and thus in the $\frac{15}{80}$-inch mirror have the opportunity of examining a field of about nine square degrees; but it was found impossible, inasmuch as the images, even towards the centre of the plate, were found to be impressed with a white centre. To a certain extent, these malformations were predicted in a paper by General Tennant in the 'Monthly Notices of the Royal Astronomical Society.'

This phenomenon necessitated the abandonment of so large a plate with its circular carrier of seven and a half inches diameter, for a smaller plate and smaller carrier having an angular field of 1° 56′ or nearly four square degrees. With this plate the images became round in the centre, and continued so to a distance of about 40′ from the centre. Then they became decidedly elliptical, having their extremities remote from the centre fainter than the opposite extremities. At the edge of the plate, the figure of the star on the side remote from the centre appeared to be not closed at all, but presented the appearance of a fan. I have, however, not observed the focal lines at right angles to each other, as seen and described by the Astronomer Royal. In the $\frac{15}{120}$-inch mirror and 4-inch plate, which presents also a field of nearly four square degrees, the phenomena here described are generally very much less pronounced, and commence at a greater distance from the centre.

The conclusions which I feel disposed to draw from the foregoing remarks, are the general unsuitability of mirrors of short focal length, and the impossibility of obtaining a large angular field in such mirrors, of a character serviceable for charting the heavens by means of photography. How far this difficulty may be obviated in refractors suitably corrected, and of comparatively short focal length, it is beyond my experience to indicate. Before instituting this trial, I had some hope, that with so simple an optical appliance as a mirror, a much larger available field might have been practically secured than has proved to be the case. I apprehend, however, that in point of light, that is, having regard alone to the faintness of the stars which, *cæteris paribus*, can be photographed, the advantage is practically on the side of the reflector.

Another point of some importance in the character of the images

impressed by these mirrors is the tendency of those formed from bright stars, to spread themselves over a larger portion of the film in the short focus mirror, and consequently to increase the difficulty of bisection. In the smaller stars, this peculiarity is not so apparent. I am here, contrary to my wont, unable to appeal to numerical data, so essentially necessary in discussions of this description, and where mere estimates and impressions are apt to mislead the judgment. The impossibility of procuring photographs of the same star from the two mirrors under exactly similar circumstances, and therefore of eliminating the relative amount of sensitiveness of the plates employed, the character of the night, and many other circumstances which occur in stellar photography, render the test of numbers impracticable. I state here the experience gained from the examination of many photographs; and in immediate connexion with this point of experience, I may mention that the conclusion has been forced upon me, that the images formed from a de la Rue metallic mirror are harder and less extended than those formed from equal exposures on a silver on glass mirror. If I were to hazard an opinion, expressed not without reserve, I should say that the difference between the action of a metallic mirror and a silver on glass mirror, may not unfitly be compared to the difference between the action of a metallic mirror, and the action of such photographic object-glasses as have come under my own observation.

## II. *The Relative Luminosity of the Images of Stars, formed by the Two Mirrors.*

The mirrors were originally silvered by Mr. Browning, about March 19th, 1887. They were in constant use until January 26th, 1888, and on that date the $\frac{15}{120}$-inch mirror was examined as to its light-reflecting capacity. The secondary plane reflector was silver on glass. The method of determination was the comparison of the places of extinction in the wedge photometer of three stars viewed respectively in the $\frac{15}{120}$-inch mirror, in the $12\frac{1}{4}$-inch Grubb refractor, and in the 4-inch finder attached to the latter. Each star was extinguished five times in each observation. The method of computation adopted in the light comparison was that explained in the 'Memoirs of the Royal Astronomical Society,' vol. 47.

The results are as follows :—

$$\text{I.}\quad \frac{\text{Light reflected by } \frac{15}{120}\text{-inch mirror}}{\text{Light transmitted by } 12\frac{1}{4}\text{-inch refractor}} = 1{\cdot}18.$$

$$\text{II.}\quad \frac{\text{Light reflected by } \frac{15}{120}\text{-inch mirror}}{\text{Light transmitted by 4-inch refractor}} = 9{\cdot}15.$$

This *mirror was subsequently* re-silvered at the Observatory by *Mr. Jenkins, the film deposited* being excellent, February 6th, 1888,

and the light was re-determined by the same method, and the same stars, on March 3rd, 1888, the weather admitting of no earlier trial. Result:—

$$\text{III.}\quad \frac{\text{Light reflected by } \tfrac{15}{120}\text{-inch mirror}}{\text{Light transmitted by } 12\tfrac{1}{4}\text{-inch refractor}} = 1{\cdot}20.$$

$$\text{IV.}\quad \frac{\text{Light reflected by } \tfrac{15}{120}\text{-inch mirror}}{\text{Light transmitted by 4-inch refractor}} = 9{\cdot}72.$$

*The $\tfrac{15}{80}$-inch Mirror.*

Determination by the process explained above, on January 3rd, 1888, of the light reflected by the $\tfrac{15}{80}$-inch mirror. Result:—

$$\text{V.}\quad \frac{\text{Light reflected by the } \tfrac{15}{80}\text{-inch mirror}}{\text{Light transmitted by the } 12\tfrac{1}{4}\text{-inch refractor}} = 1{\cdot}23.$$

$$\text{VI.}\quad \frac{\text{Light reflected by the } \tfrac{15}{80}\text{-inch mirror}}{\text{Light transmitted by the 4-inch refractor}} = 10.$$

This mirror was re-silvered at the Observatory by Mr. Jenkins on January 9th, 1888, and re-examined on January 17th, 1888. With the results—

$$\text{VII.}\quad \frac{\text{Light reflected by } \tfrac{15}{80}\text{-inch mirror}}{\text{Light transmitted by } 12\tfrac{1}{4}\text{-inch refractor}} = 1{\cdot}33.$$

$$\text{VIII.}\quad \frac{\text{Light reflected by } \tfrac{15}{80}\text{-inch mirror}}{\text{Light transmitted by 4-inch refractor}} = 10{\cdot}70.$$

On combining the above results, it appears that by means of the comparisons with the $12\tfrac{1}{4}$-inch refractor—

$$\text{IX.}\quad \frac{\text{Light of } \tfrac{15}{80}\text{-inch mirror re-silvered}}{\text{Light of } \tfrac{15}{80}\text{-inch mirror after 9 months' use}} = \frac{1{\cdot}33}{1{\cdot}23} = 1{\cdot}08,$$

and from comparison made with the 4-inch refractor—

$$\text{X.}\quad \frac{\text{Light of } \tfrac{15}{80}\text{-inch re-silvered}}{\text{Light of } \tfrac{15}{80}\text{-inch mirror after 9 months' use}} = \frac{10{\cdot}7}{10} = 1{\cdot}07.$$

In like manner, from similar processes with respect to the $\tfrac{15}{120}$-inch mirror, it appears that when the comparisons were made by the aid of the $12\tfrac{1}{4}$-inch refractor—

$$\text{XI.}\quad \frac{\text{Light of } \tfrac{15}{120}\text{-inch mirror re-silvered}}{\text{Light of } \tfrac{15}{120}\text{-inch mirror after 9 months' use}} = \frac{120}{118} = 1{\cdot}01,$$

and when compared by means of the 4-inch refractor—

$$\text{XII.}\quad \frac{\text{Light of } \tfrac{15}{120}\text{-inch mirror re-silvered}}{\text{Light of } \tfrac{15}{120}\text{-inch mirror after 9 months' use}} = \frac{9{\cdot}72}{9{\cdot}15} = 1{\cdot}06.$$

*The approximate* identity of the above results is, I think, such as *to commend the* method adopted with the wedge photometer to con-

fidence, inasmuch as these small discrepancies are well within the limits of the errors of observation.

The conclusions to be drawn from these results thus obtained are: 1. The very slight deterioration of the mirrors after nine months' constant use and exposure. 2. The very considerable amount of light reflected by these mirrors when compared with that transmitted by the Grubb object-glass, amounting in fact to this, that a mirror of 15 inches aperture affords an image of a star as brilliant as that formed by an object-glass (of the particular quality presented) of 13·35 inches aperture. 3. A slightly increased, but only a slightly increased, luminosity of image is caused by the adoption of the focal length of 80 inches instead of 120. The result, referred to above in 2, is in conformity with the remark made by Dr. Robinson, in 'Phil. Trans.,' vol. 151, to the effect that in respect of the luminosity of the image, Newtonian reflecting telescopes of attainable aperture would probably surpass refractors of attainable dimensions, on account of the increasing absorption of light, by reason of thickness, unless indeed the translucency of glass can be sensibly improved.

It is to be noticed that with an exposure of half an hour in the $\frac{15}{80}$-inch mirror, the existence of nebulosity in the neighbourhood of Maia is distinctly traceable on the photographic plate. With the exposure of an hour it is observable in form. No trace of the fainter nebulosity near Merope has been impressed.

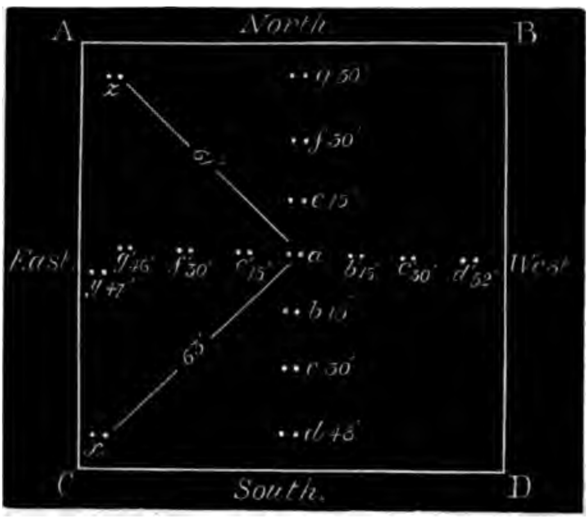
### III. *The Angular Extent of Apparently Undistorted Field, and the Amount of Distortion where it Exists.*

The determination of these elements is of the utmost importance in the formation of charts of the heavens by the aid of photography, inasmuch as on the superficial extent of the reliability of the photographic field depend the time, labour, and cost of charting the heavens. A general idea of this extent of reliable field may be gathered from the quality of the stellar images impressed at different distances from the centre of the plate. Thus in the case of the $\frac{15}{80}$-inch mirror at about forty minutes from the centre of the plate the star images cease to be sufficiently circular, although for a short distance beyond, distances between these stellar disks may still be measured, though not possessing the utmost reliability. In the case of the $\frac{15}{120}$-inch mirror, this angular extent of measurable field extends beyond fifty-one minutes from the centre. This very perceptible superiority of the $\frac{15}{120}$-inch mirror arises, partly, no doubt, from its longer focus, and it may also be influenced by the effects of the intervention of the plate holder; but be the causes what they may, the superiority longer focus is unquestionable in respect of extent of field.

But an *equally important* element remains to be investigated, *namely, the amount of distortion* which exists at different distances

from the centre of the plate, and in order to ascertain this, I made a preliminary examination of the optical quality of the field, by the method which I have described in vol. 47 of the 'Memoirs of the Royal Astronomical Society' (p. 238). This method consists in shifting the images of the same pair of stars to widely different localities in the field of view, and it was argued that so long as the measured angular distances between these pairs remained sensibly the same, *i.e.*, within the known and unavoidable limits of observational error, so long might the optical field of view be relied upon as sensibly accurate.

FIG. 1.




ABCD represents the photographic plate where AB is 4 inches, and subtends an angle of 1° 55′ at the centre of the $\frac{15}{120}$-inch mirror. A pair of stars of approximately the seventh magnitude was selected, and photographed near the centre of the plate, as at (*a*), with an exposure of five minutes. The telescope was then moved approximately fifteen minutes to the south, and a second photograph taken, by which this same pair was removed to (*b*). This process was repeated again and again in northerly, easterly, and westerly directions, till after thirteen exposures this same pair of stars was dotted about the plate as in the diagram. This same process was repeated on three plates on the same night (March 3, 1888). The distances between each pair were then measured, and the means of *five measures of each* pair were taken as the adopted measures for each *pair respectively*. The results are as follows:—

Distances between the Pair of Stars, corrected for Refraction.

| Position of star.. | *a.* | *b.* | *c.* | *d.* | *e.* | *f.* | *g.* |
|---|---|---|---|---|---|---|---|
| Plate I.. | 152″·00 | 151″·92 | 151″·93 | 151″·99 | 152″·07 | 152″·22 | 152″·02 |
| II.. | 151·88 | 2·07 | 1·81 | 152·10 | 1·88 | 1·95 | 2·02 |
| III.. | 152·08 | 2·17 | 2·05 | 2·03 | 1·96 | 2·07 | 2·44 |
| Mean.. | 151·99 | 152·05 | 151·93 | 152·04 | 151·97 | 152·08 | 152·16 |
| Position of star.. | *a.* | *b′.* | *c′.* | *d′.* | *e′.* | *f′.* | *g′.* |
| Plate I.. | 152″·00 | 152″·14 | 152″·05 | 152″·17 | 151″·89 | 151″·91 | 152″·05 |
| II.. | 151·88 | 2·19 | 2·08 | 2·36 | 1·83 | 1·96 | 1·86 |
| III.. | 152·08 | 2·07 | 2·00 | 2·24 | 2·05 | 1·76 | 2·10 |
| Mean... | 151·99 | 152·13 | 152·04 | 152·26 | 151·92 | 151·88 | 152·00 |

The following table exhibits the deviations of the intervals from the central interval at different positions on the plate:—

| | | |
|---|---|---|
| Due North | 15′ | −0″·02 |
| | 30 | +0·09 |
| | 50 | +0·17 |
| Due South | 15 | +0·06 |
| | 30 | −0·06 |
| | 48 | +0·05 |
| Due East | 15 | −0·07 |
| | 30 | −0·11 |
| | 46 | +0·01 |
| Due West | 15 | +0·14 |
| | 30 | +0·05 |
| | 52 | +0·27 |

When it is remembered that the unavoidable error of such measures is about 0″·2 (where 0·0001 in. is equivalent to 0″·17), the only conclusion to be drawn is that to the extent of the field impressed on this plate of 1° 55′ square, there is no perceptible or measurable distortion in the apparent distance of these pairs, and in fact that small measured distances may be relied upon throughout the field; and thus, if a few stars are scattered about the plate with known coordinates, *those of all the rest* may be conveniently determined with *great accuracy*.

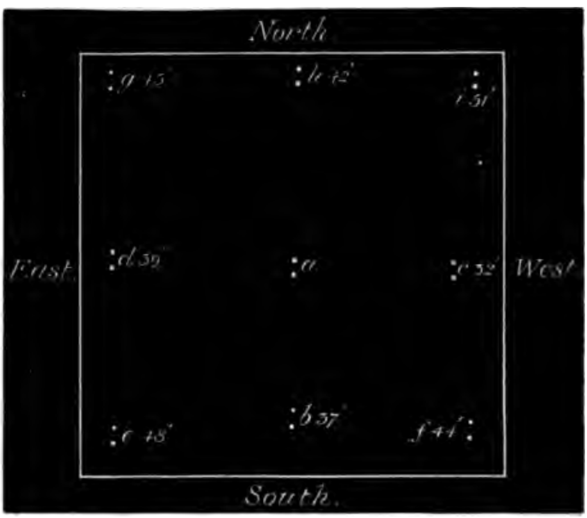
Subsequently to the above operation it was thought well to examine a pair of stars on the same plates, which happened to fall near the angles, viz., at ($x$) and ($z$), impressions of the same pair as those at $y$. The distance of ($x$) and ($z$) from the centre of the plate was approximately 63′, but it must be added that the impressed disks were slightly elliptical. The resulting distances between the stars of these three pairs were as follows :—

| | $x$. | $y$. | $z$. |
|---|---|---|---|
| Plate I ...... | 176″·53 .... | 176″·11 .... | 176″·35 |
| II ...... | 6·17 .... | 6·10 .... | 176·29 |
| III ...... | 6·22 .... | 6·64 .... | 176·15 |

*Examination of the Field of the $\frac{15}{80}$-inch Mirror.*

The photographic plate here is nearly 3 inches square, subtending an angle of 1° 56′. The pair of stars selected consisted of Atlas and Pleione, and these by the motion of the telescope were made to occupy successively the positions indicated in the subjoined diagram, which will be understood from the description of the former. The tables are arranged on the same plan.

FIG. 2.



Distances of Atlas from Pleione, corrected for Refraction.

| Position of star.... | *a.* | *b.* | *c.* | *d.* | *e.* |
|---|---|---|---|---|---|
| Plate I ........ | 301″·21 | 301″·47 | 301″·52 | 301″·29 | 301″·63 |
| II ........ | 1·15 | 1·53 | 1·74 | 1·53 | 1·80 |
| III ........ | 1·40 | 1·32 | 1·60 | 1·47 | 1·52 |
| Mean........ | 301·25 | 301·44 | 301·62 | 301·43 | 301·65 |
| Position of star.... | *f.* | *g.* | *h.* | *i.* | |
| Plate I ........ | 301″·37 | 301″·32 | 301″·58 | 302″·17 | |
| II ........ | 1·41 | 1·60 | 1·79 | 2·01 | |
| III ........ | 1·29 | 1·39 | 1·71 | 1·92 | |
| Mean........ | 301·36 | 301·41 | 301·69 | 302·03 | |

The following table exhibits the deviations of the intervals from the central interval at different positions on the plate:—

| | | |
|---|---|---|
| Due North | 42′ | +0″·44 |
| N. West | 51 | +0·78 |
| Due West | 32 | +0·37 |
| S. West | 44 | +0·11 |
| Due South | 37 | +0·19 |
| S. East | 48 | +0·40 |
| Due East | 39 | +0·18 |
| N. East | 45 | +0·19 |

It should be observed here, that while the linear discrepancies of measured distances are the same as those with the $\frac{15}{120}$-inch mirror, they indicate larger angular discrepancies in the ratio of 3 : 2. Nevertheless, the examination of these angular discrepancies exhibits evident traces of distortion, sufficient to render extreme accuracy of measures unattainable without the great difficulty of an extensive tabulation; in other words, the comparative short focus of this mirror is not well adapted to the purposes of accurate measurement. Perhaps I ought here to refer to the very careful examination of the field of the Grubb refractor of 12¼ inches aperture and 176 inches focal length, recorded in the 'Memoirs of the Royal Astronomical Society,' vol. 47, p. 238, in which it appears that no absolute reliance could be assigned to measures extending beyond 12 minutes from the

centre of the field, that is to say, beyond a field whose diameter exceeds 1400″.

Over and above this question of the accurate measurement of small distances from stars of known co-ordinates scattered about the field, there is the question of the possibility of accurate measurement of considerable distances from the centre of the plate itself. In other words, can a linear measure on a photographic plate be accurately translated into the corresponding angular distance between two stars by simple multiplication by a constant? In order to investigate this very important question, I had a series of measures made of sixteen stars of the Pleiades from the star ($p$) Pleiadum, compared with the corresponding heliometer measures, as given by Dr. Elkin in the Yale College publications. These distances extend from 400″ to 3200″. The form which this examination assumed was that proposed by Dr. Gill in the 'Bulletin du Comité International Permanent pour l'Exécution Photographique de la Carte du Ciel,' Paris, 1888, in which the heliometer distance ($s$) between two given stars is equated to—

$$as + bs^2 + cs^3 + \&c.,$$

where ($s$) is the distance, measured on the plate in *inches*. This investigation was first applied to the shorter focus mirror, inasmuch as it was expected to indicate sensible discrepancies from an uniform scale. The solution of the equations of condition give the following form for the conversion of the linear distance ($s$) into angular measure:—

$$2577''\cdot0396\, s + 0''\cdot4546\, s^2.$$

The probable error of the coefficient of $s^2$ is $\pm$ 0″·2831, indicating an amount of insecurity which renders this method of investigation somewhat doubtful; but taking it as it stands, this formula shows that while in a measured distance of half an inch, equivalent to 1200″, no measurable error beyond 0″·1 is introduced, yet in a measure of 2 inches from the centre there is a possible or even probable correction to be made, amounting to nearly two seconds. This seems to indicate the absolute necessity of a rigid investigation of the photographic field of all instruments in which that field is extensive.

A similar enquiry, referred also to Dr. Elkin's heliometer measures, was made though on a more restricted field, in the case of the de la Rue mirror, which has already been so extensively used for exact astronomy. In this case the coefficient ($b$) of the term depending on the square of the linear distance ($s$) inches, is

$$+\,0''\cdot333 \pm 0''\cdot202,$$

and, inasmuch as the measures actually made use of hitherto have never exceeded 0·75 inch from the centre of the field, this correction (admitting its reality) indicates an uncertainty of about 0″·16. In the method employed for parallax determinations with this instrument, this source of error, small as it is, is effectually eliminated by the avoidance of all but differential measures.

IV. *The Photographic Capacities of the Two Mirrors in respect of the Faintest Stars impressed on Plates with Exposures of given Duration.*

The method employed was that described in the 'Proceedings of the Royal Society,' No. 247 (read May, 1886). It consisted in taking with each of the two mirrors three plates of the Pleiades exposed for 5, 30, and 60 minutes respectively. The diameters of a few stars whose magnitude had been well determined by the wedge photometer were measured five times on each of the plates, and then by the means indicated in the above-mentioned paper, the following results were obtained:—

Mirror $\frac{15}{120}$-inch.

Exposures of 5, 30, and 60 minutes, respectively, gave—

5 min.:—log mag. required = log 11·14 (mag.) − 0·0204 $\delta$.
30 min.:—log mag. required = log 13·55 (mag.) − 0·0203 $\delta$.
60 min.:—log mag. required = log 14·79 (mag.) − 0·0193 $\delta$.

In the above formula log 14·79 indicates the magnitude of the faintest star just beginning to be impressed on the photographic plate during its exposure of 60 minutes. This number and the coefficient of $\delta$ were obtained in the manner already referred to above, where $\delta$ is the measured diameter of the star whose magnitude is sought, expressed in seconds of arc.

In like manner, the magnitude of the faintest star, during an exposure of 30 minutes, was 13·55 maguitude, and during an exposure of 5 minutes, was 11·14 magnitude.

Mirror $\frac{15}{80}$-inch.

A similar investigation applied to this mirror gave the following results after exposures of similar duration:—

5 min.:—log mag. required = log 11·93 − 0·0215 $\delta$.
30 min.:—log mag. required = log 13·79 − 0·0186 $\delta$.
60 min.:—log mag. required = log 15·13 − 0·0197 $\delta$.

*From this it appears that the* photographic capacity in respect of *the faintness of the light impressed* is slightly in favour of the shorter

focus mirror, and that with an exposure of one hour no fainter star than the fifteenth magnitude leaves a trace at all discernible on the photographic film.

In the following tables are given the results of the preceding formulæ as applied to stars whose magnitudes have been determined by the wedge photometer, and recorded in the 'Uranometria Nova Oxoniensis.' In the first column is given the designation of the star in the Pleiades, adopted by Bessel. The remaining columns speak for themselves.

Table I.—Exposure 5 minutes. Mirror $\frac{15}{80}$-inch.

| Star's designation. | Measured diameter. | Computed (photographic) magnitude. | Photometric magnitude U.N.O. | Difference C − O in mag. |
|---|---|---|---|---|
| No. 8...... | 10″·01 | 7·27 | 7·36 | −0·09 |
| 35...... | 4·75 | 9·43 | 9·67 | −0·24 |
| 40...... | 9·89 | 7·31 | 7·17 | +0·14 |
| 22...... | 11·65 | 6·70 | 6·80 | −0·10 |

Table II.—Exposure 30 minutes.

| Star's designation. | Measured diameter. | Computed (photographic) magnitude. | Photometric magnitude U.N.O. | Difference C − O in mag. |
|---|---|---|---|---|
| No. 8...... | 14″·41 | 7·44 | 7·36 | +0·08 |
| 35...... | 8·68 | 9·51 | 9·67 | −0·16 |
| 40...... | 15·11 | 7·22 | 7·17 | +0·05 |
| 22...... | 16·61 | 6·77 | 6·80 | −0·03 |

Table III.—Exposure 60 minutes.

| Star's designation. | Measured diameter. | Computed (photographic) magnitude. | Photometric magnitude U.N.O. | Difference C − O in mag. |
|---|---|---|---|---|
| No. 8...... | 15″·97 | 7·33 | 7·36 | −0·03 |
| 35...... | 10·45 | 9·42 | 9·67 | −0·25 |
| 40...... | 16·64 | 7·11 | 7·17 | −0·06 |
| 22...... | 17·28 | 6·91 | 6·80 | +0·11 |

Attention may here be drawn to the precision of the results ained by measures so independent of each other. As an accidental ult of these recent measures of the photographic magnitude of the rs, it may be mentioned that in May, 1886, the photographic gnitude of Star 22 in the Pleiades was 0·35 magnitude less n the photometric as obtained from very many measures, and I ributed this difference to the probable actinic peculiarity of the r in question, but inasmuch as no such perceptible difference exists the more recent measures of the photographic and photometric gnitudes, resulting as they do from so many independent deter- nations, the question of the variability of this star is suggested as y probable. Pleione also in the measures of 1886 exhibits a erence between the photometric and the photographic magnitude. smuch as the same difference in the measures has been again ibited in the recent measures, it seems reasonable to explain the t by the peculiar actinic action in the light of this star.

As a further example of the power and applicability of this finite method in reference to faint stars not suitable for determina- n by the wedge photometer, I may add here the following nparison of the resulting *measures* made by the photographic thod, set side by side with the magnitude as *estimated* by Wolf Description du Groupe des Pléiades,' Paris, 1874).

| Star's designation No. in Wolf. | Measured diameter. | Computed (photographic) magnitude. | Estimated magnitude. Wolf. | Difference C − O in mag. |
|---|---|---|---|---|
| 196 | 9″·75 | 9·72 | 10 | −0·28 |
| 314 | 9·98 | 9·61 | 10 | −0·39 |
| 239 | 6·04 | 11·51 | 11 | +0·51 |
| 241 | 5·85 | 11·60 | 11 | +0·60 |
| 318 | 8·15 | 10·45 | 11 | −0·55 |
| 319 | 8·40 | 10·34 | 11 | −0·66 |
| 325 | 5·67 | 11·70 | 12 | −0·30 |
| 330 | 6·14 | 11·45 | 12 | −0·55 |
| 331 | 5·35 | 11·87 | 12 | −0·13 |
| 320 | 4·47 | 12·35 | 13 | −0·65 |
| 321 | 3·98 | 12·63 | 13 | −0·37 |
| 332 | 4·23 | 12·49 | 13 | −0·51 |
| 302 | 3·50 | 12·91 | 14 | −1·09 |
| 324 | doubtful | .. | 14 | .. |

t has been more than once proposed to estimate or to measure the tographic magnitudes of stars, by means of the breadth and racter *of their traces on the* photographic plates. This method *ld involve an unnecessary* consumption of time in procuring

impressions made with this object in view alone. But by the method here adopted, the same plates which are taken for ascertaining the co-ordinates of the stars, serve equally well for measuring their photographic magnitudes. It is perhaps unnecessary to point out that practically the photometric and photographic magnitudes are, for the most part, identical. The remark above will fail of application, if it be possible to determine differences of right ascension and of declination from the traces of the stars with sufficient accuracy.

## VI. "On the Development of Voltaic Electricity by Atmospheric Oxidation." By C. R. Alder Wright, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. Thompson, F.I.C., F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received April 17, 1888.

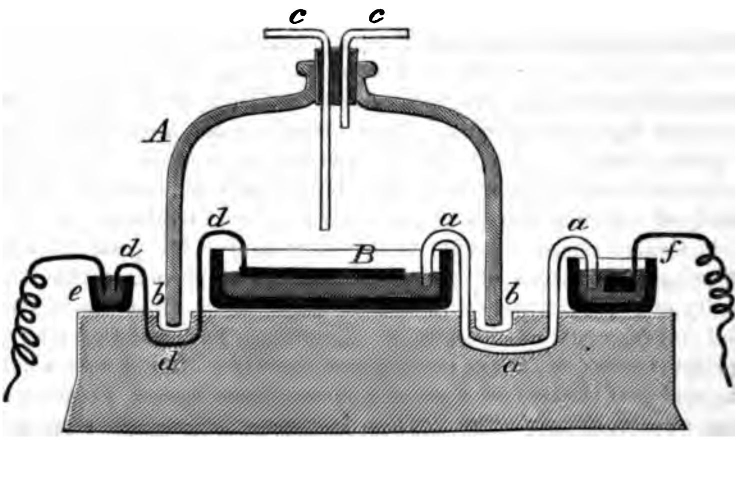
In a preliminary note on this subject ('Roy. Soc. Proc.,' vol. 42, p. 212), it has been shown that when copper is immersed in an aqueous solution of ammonia and opposed to an "aëration plate" of some conducting material not otherwise acted upon, lying horizontally on the surface of the fluid, a current flows continuously through a wire, &c., made to connect the two plates, the energy manifested by which is due to the absorption of atmospheric oxygen by the aëration plate and the indirect combination of this with the copper forming cuprous oxide which dissolves in the ammonia. Numerous analogous electromotor cells are readily obtainable by suitably varying the metal susceptible of oxidation and the electrolytic fluid employed, some of which we have submitted to close examination; whilst another class of voltaic cells, acting on much the same principle, we find can be obtained by substituting for the oxidisable metal a platinum or other incorrodible plate immersed in an oxidisable fluid, such as pyrogallol dissolved in caustic soda: preferably the aëration plate is arranged in one vessel on the surface of some convenient fluid (not necessarily identical with the oxidisable one), and the other plate and oxidisable fluid placed in another vessel, the two being connected by a siphon or wet wick; or the whole may be arranged as a gravity battery, the oxidisable fluid being made the heavier one so as to preserve it from direct contact with air; or a U-tube arrangement may be employed. Thus, for example, a platinum plate immersed in an acid solution of ferrous sulphate, or in sulphurous acid solution, connected with a vessel containing dilute sulphuric acid, and an aëration plate of spongy platinum, &c., furnishes an electromotor cell in which the *production of a current* is accompanied by the virtual transference *of oxygen from* the aëration plate to the oxidisable fluid, forming

c sulphate or sulphuric acid respectively. A considerable ncement in the E.M.F. of such an arrangement is effected if the isable fluid is rendered *alkaline*, and the aëration plate surrounded *cid* fluid, as in that case the passage of a current is also accom-ed by the mutual neutralisation of the acid and alkali to an nt equivalent to the current flowing (apart from diffusion): the of neutralisation of caustic soda and sulphuric acid is (Julius msen) 31,378 gram-degrees, equivalent to 0·675 volt; and larly in other cases. Thus, tolerably energetic aëration cells are ined by using caustic soda solution of pyrogallol or sodium sulphite (Schützenberger's *hydrosulphite*), and a platinum sponge tion plate on the surface of diluted sulphuric acid; and somewhat ker ones if an alkaline solution of ferro-cyanide or sodium plumbate ised, ferri-cyanide or lead dioxide (precipitated in the solid form he electrode) being the product of the oxidation thus effected, the E.M.F. being upwards of 0·8 volt in each case.

n all aëration cells, whether oxidisable fluids or metals be employed, marked feature is the extremely rapid rate at which the E.M.F. he cell falls if the current generated is made to exceed a very ll limiting density relatively to the area of the aëration plate. course when this plate is a sheet of polished metal such as platinum this limit lies much lower than when it is a tray of the same filled with spongy metal, pulverised graphite, &c., because in the er case the true surface acting is much greater than the actual of the tray: a number of observations led us to the conclusion with the most sensitive kinds of aëration plates examined (thin s or leaves of the precious metals), a measurable depreciation in the I.F. of a cell that otherwise would give a constant value, was edily brought about if made to generate a current of greater sity relatively to the aëration plate than about $\frac{1}{25}$ micro-ampère square centimetre of surface, or 1 micro-ampère for a plate entimetres square, exposing 25 square centimetres of surface (one only reckoned); but with aëration plates of spongy metal rents of many times this density produced little or no depreciation n after flowing some time. Even with the most favourable kinds plates, however, the tendency towards depreciation was so far ked as to render it evident that but little hope could be enter-ed of utilising the principle of atmospheric oxidation for the duction of cheap currents of sufficient power for practical use, epting when plates of enormous area are employed, so that the sity of the current should still be small, even when the current lf was of moderate magnitude. Admitting, however, that a large erficial area (*e.g.*, a lake or artificial reservoir) of fluid were avail-e, and that *the cost of a proportionately* large system of aëration *es were not prohibitive, it does* not seem absolutely impossible

that the production of currents by atmospheric oxidation might be practically effected on the large scale.

We found it difficult to obtain sharp and concordant valuations of the E.M.F. actually set up in cells containing oxidisable fluids, the more so, as the numbers appeared to vary, not only with the nature of the aëration plate and the fluid in contact therewith, but also with the character of the incorrodible plate immersed in the oxidisable fluid, and with the nature and strength of that fluid also. With cells containing oxidisable metals, however, and electrolytic fluids in which the oxides formed were soluble, we found no difficulty in obtaining far more concordant and approximately constant values than would at first sight have appeared likely, or even possible with combinations in which one ingredient was so unstable an element as a film of gaseous matter attracted to the surface of a condensing solid, and simultaneously in contact with a fluid capable of dissolving the gas. Obviously, mechanical disturbances, rapid alterations of temperature, and such like causes would be likely to cause large variations from time to time in the readings of any one particular cell; whilst unavoidable differences in the conditions of surface of otherwise duplicate plates (such as variations in degree of polish, &c.) would render it likely that the average readings of any two duplicate cells would occasionally exhibit considerable divergence; we succeeded, however, in reducing these sources of fluctuations to comparatively small limits, by setting up the cells in an apartment where the temperature varied but little, and only slowly, the readings being mostly taken in the mornings after standing at rest all night; whilst alteration of the fluid by evaporation, attraction of moisture, carbonic acid, &c., from the air, falling in of dust, and so on, was avoided as much as possible by covering over the vessel containing the aëration plate with a bell-jar



A, the siphon connecting this vessel with the other one in which the oxidisable metal was immersed being bent so as to pass under the rim of the jar *a*, *a*, *a*. It was found convenient to mount the jar on a block of paraffin wax with a circular groove *b*, *b*, in which the bell-jar stood, the groove being then filled with mercury so as to make a sort of hydraulic lute; if required, the air inside the jar could be replaced by oxygen, &c., by simply passing in a current of gas through one of a pair of tubes *c*, *c*, introduced through a perforated cork in the neck of the jar. Usually several aëration plates were separately arranged in the same vessel, each one, B, being connected (by means of a platinum wire *d*, *d*, *d*, imbedded in the paraffin wax, passing under the rim of the jar), with a mercury cup *e* outside: in this way the plate of oxidisable metal used, *f*, could be removed at pleasure for cleaning, amalgamating, &c., and replaced without disturbing the aëration plates, and could be opposed at will to any one of these by a simple switch connecting the required mercury cup with the rest of the circuit.

On first setting up such an arrangement and taking readings alternately with any one of the plates opposed to the oxidisable metal, and a Clark's cell, the total resistance in circuit being the same (usually several megohms to reduce the current density sufficiently) values were obtained generally exhibiting progressive alteration (sometimes increase, sometimes decrease) as time elapsed; but after periods varying in different cases from an hour or two to several days, sensibly steady readings were obtained exhibiting little or no variation for days and even weeks together; what variations were observed were generally traceable either to temperature fluctuations or to slight shaking or mechanical disturbance whilst renewing the opposed oxidisable plate, or to slight differences in the latter. If, however, in cells set up with dilute sulphuric acid air had free access, more or less considerable alteration was often brought about after some time through evaporation or attraction of moisture from the air, altering the film of fluid in contact with the aëration plate; and this was still more the case with cells set up with caustic soda solution through absorption of carbonic acid, and with ammonia cells through volatilisation of ammonia.

The result of a large number of observations with cells of various kinds was to show that the following general proposition holds:—

If a cell set up with a given fluid, oxidisable metal, and aëration plate generate an E.M.F. $= e_1$, then the effect of substituting another aëration plate for the first is to alter the E.M.F. to $e_2 = e_1 + K_1$; whilst that of substituting a different oxidisable metal is to alter the E.M.F. to $e_3 = e_1 + K_2$; the quantity $K_1$ being independent of the *nature of the oxidisable* metal used, but varying with *each kind of aëration plate* employed, and also to some extent with

the nature of the fluid; and similarly the quantity $K_2$ being independent of the nature of the aëration plate used, but varying with each kind of oxidisable metal employed, and to some extent also with the nature of the fluid.

For example, in one experiment four aëration plates, respectively platinum sponge, gold sponge, silver sponge, and graphite, were successively opposed, first to amalgamated zinc, and then to brightened lead in a caustic soda solution of strength $3 \cdot 45 Na_2O, 100 H_2O$, giving the following average values after making a long series of readings (Clark's cell = 1·435 at 15° C.) :—

| | Zinc. | Lead. | Difference = $K_2$. |
|---|---|---|---|
| Platinum sponge............. | 1·471 | 0·769 | −0·702 |
| Gold sponge.................. | 1·435 | 0·732 | −0·703 |
| Silver sponge................ | 1·619 | 0·916 | −0·703 |
| Graphite..................... | 1·400 | 0·696 | −0·704 |
| | | Mean.. | −0·703 |

| Values of $K_1$. | Zinc. | Lead. | Mean. |
|---|---|---|---|
| Platinum sponge replaced by gold sponge........................ | −0·036 | −0·037 | −0·0365 |
| Platinum sponge replaced by silver sponge........................ | +0·148 | +0·147 | +0·1475 |
| Platinum sponge replaced by graphite | −0·071 | −0·073 | −0·072 |
| Gold sponge replaced by silver sponge........................ | +0·184 | +0·184 | +0·184 |
| Gold sponge replaced by graphite... | −0·035 | −0·036 | −0·0355 |
| Silver sponge replaced by graphite... | −0·219 | −0·220 | −0·2195 |

Numerous other experiments of the same kind were made with analogous results in all cases; the values for $K_1$ and $K_2$ respectively found in any given set of observations never differing by quantities outside the limits of experimental error. The average values of $K_1$ or $K_2$ thus deduced for a given fluid, however, always differed measurably from those similarly deduced for a different fluid, even when that was a similar solution but of different strength. The tables hereafter described illustrate these differences more fully.

*Cells set up with Caustic Soda Solution as Electrolytic Fluid, and various Aëration Plates opposed to Zinc.*

When steadiness was once obtained, we found that the fluctuations observed from day to day in a given cell set up with amalgamated zinc as oxidisable metal (freshly amalgamated each day) rarely exceeded $\pm$ 0·003 to 0·004 volt difference from the mean of several days (sometimes some weeks) readings. Duplicate cells, however, gave average readings exhibiting greater differences up to $\pm$ 0·025 or $\pm$ 0·030 volt: thus eighteen different cells set up with platinum-foil or thin leaf, caustic soda solution of strength $3{\cdot}45Na_2O,100H_2O$, and amalgamated zinc gave the following results:—

| | |
|---|---|
| Maximum mean reading of any given cell ..... | 1·445 |
| Minimum mean reading of any given cell...... | 1·403 |
| Average reading of all...................... | 1·423 |
| Probable error of average.................. | $\pm$0·0019 |

Even with a much smaller number of cells, the probable error was usually well within $\pm$ 0·005 volt, the differences observed with different cells mainly depending on the unavoidably slight differences in the surface of the metal, &c., constituting the aëration plate.

On substituting a stronger solution of caustic soda for a weaker one, as a rule an increment in average value was observed, and *vice versâ;* but the extent of the alteration varied considerably with different kinds of aëration plates: with solutions of strength $0{\cdot}05Na_2O,100H_2O$ the readings fluctuated so irregularly as to prevent any approach to an accurate average valuation; but with stronger solutions the readings were sufficiently concordant to reduce the probable error of the final average to only $\pm$ a few millivolts.

Cells set up with $3{\cdot}45Na_2O,100H_2O$.

| Aëration plate. | No. of cells. | Maximum. | Minimum. | Average. |
|---|---|---|---|---|
| Silver sponge (from acetate) .. | 8 | 1·624 | 1·615 | 1·618 |
| Palladium sponge ........... | 4 | 1·569 | 1·549 | 1·563 |
| Silver sponge (from chloride) . | 4 | 1·551 | 1·455 | 1·484 |
| Platinum sponge ............ | 14 | 1·491 | 1·450 | 1·463 |
| Palladium foil .............. | 4 | 1·468 | 1·422 | 1·448 |
| Gold sponge................. | 10 | 1·450 | 1·433 | 1·443 |
| Graphite (natural) .......... | 9 | 1·465 | 1·392 | 1·428 |
| Gold leaf and foil ........... | 16 | 1·449 | 1·402 | 1·426 |
| Platinum leaf and foil ....... | 18 | 1·445 | 1·403 | 1·423 |
| Silver leaf and foil........... | 21 | 1·425 | 1·367 | 1·396 |
| Carbon, Specimen A ......... | 4 | 1·383 | 1·344 | 1·365 |
| Carbon, Specimen B......... | 4 | 1·307 | 1·269 | 1·287 |

The spongy metals used were prepared as follows:—Silver sponge (from acetate) by gently igniting in the air crystallised silver acetate; that from chloride by boiling well-washed silver chloride with sugar and caustic soda until reduction was nearly complete. Spongy palladium and platinum by gentle ignition of the ammonio-chlorides of palladium and platinum respectively; and spongy gold by gentle ignition in the air for a long time (so as to burn off carbon) of cinchonine auro-chloride. The graphite was a very pure natural specimen from Ceylon; when used it was coarsely powdered, and spread over the surface of porous earthenware like the spongy metals. As regards the leaves and foils of silver, gold, and platinum, no discernible differences could be distinguished between the values given by the thinnest leaves and comparatively thick foils (up to 0·1 mm. in thickness) in any of the three cases, saving that the latter took a much longer time before steady readings were obtained. Carbon (A) was a piece of electric light rod ground down to a thin flat plate; (B) part of the carbon for a Leclanché cell similarly treated.

It may be noticed that some aëration plates composed of spongy platinum with a top layer of platinum-black (precipitated from the chloride by boiling with caustic soda and alcohol) gave figures pretty close to those furnished by platinum sponge; as also did other plates consisting of porous earthenware painted over with platinochloride of ammonium made into a paste with gum-water, ignited, and the film of spongy platinum left on the surface burnished bright.

| | Maximum. | Minimum. | Mean. |
|---|---|---|---|
| Platinum sponge and black.......... | 1·473 | 1·441 | 1·457 |
| Burnished pot ..................... | 1·455 | 1·453 | 1·454 |
| Platinum sponge alone ............ | 1·491 | 1·450 | 1·463 |

When dilute sulphuric acid was the fluid, however, the platinum-black plates gave values upwards of a decivolt *higher* than sponge, and the burnished pots about as much *lower* than sponge.

A large number of observations were made with sets of aëration plates and oxidisable metals in contact with caustic soda solution of one strength subsequently changed for a different one, and so on, only those readings being taken into account when steadiness was attained; thus the following figures were obtained where the plates were read first in $1·75Na_2O,100H_2O$, then in $3·45Na_2,100H_2O$, then in $7·15Na_2O$, $100H_2O$, and then in the first again, and so on several times, so that each plate was read several times in each strength of fluid. *In all cases the stronger* the solution the higher the value, but the effect of *a given increment* in solution-strength was very different with dif-

ferent aëration plates: thus the following mean readings were obtained.

Increment in E.M.F. brought about by increasing the Strength of Caustic Soda Solution from $m Na_2O,100H_2O$ to $n Na_2O,100H_2O$.

| | $m$ = 1·75. $n$ = 3·45. | $m$ = 3·45. $n$ = 7·15. | $m$ = 1·75. $n$ = 7·15. |
|---|---|---|---|
| Platinum foil | 0·033 | 0·015 | 0·048 |
| Platinum sponge | 0·029 | 0·004 | 0·033 |
| Silver foil | 0·014 | 0·032 | 0·046 |
| Silver sponge | 0·019 | 0·027 | 0·046 |
| Gold foil | 0·031 | 0·010 | 0·041 |
| Gold sponge | 0·027 | 0·016 | 0·043 |
| Palladium foil | 0·032 | 0·007 | 0·039 |
| Graphite | 0·029 | 0·001 | 0·030 |

Combining these figures with the average values previously obtained for $3·45Na_2O,100H_2O$, the following mean values result.

E.M.F. of Cells set up with Amalgamated Zinc and $m Na_2O,100H_2O$.

| | $m$ = 1·75. | $m$ = 3·45. | $m$ = 7·15. |
|---|---|---|---|
| Spongy silver (acetate) | 1·599 | 1·618 | 1·645 |
| Spongy palladium | .. | 1·563 | .. |
| Spongy silver (chloride) | .. | 1·484 | .. |
| Spongy platinum | 1·334 | 1·463 | 1·467 |
| Palladium foil | 1·416 | 1·448 | 1·455 |
| Spongy gold | 1·416 | 1·443 | 1·459 |
| Graphite | 1·399 | 1·428 | 1·429 |
| Gold foil and leaf | 1·395 | 1·426 | 1·436 |
| Platinum foil and leaf | 1·390 | 1·423 | 1·438 |
| Silver foil and leaf | 1·382 | 1·396 | 1·428 |
| Carbon (mean of A and B) | .. | 1·326 | .. |

*Cells set up with Caustic Soda Solution and Various Kinds of Aëration Plates opposed to Lead.*

It was found that when a given aëration plate had attained to a condition of steadiness, if opposed to a piece of freshly brightened pure lead, somewhat irregular readings were obtained for a few minutes owing to the effect of the alkali on the polish of the lead; in a short time, however, this disturbing influence mostly subsided, and tolerably constant values were obtained for two or three hours, after which a slight lowering generally began to be noticed con-

currently with a notable increase in the amount of corrosion of the lead; the following values were obtained during this period of two or three hours whilst the readings were comparatively constant.

Values of $K_2$ = effect of substituting Brightened Lead for Amalgamated Zinc in $m Na_2O, 100H_2O$.

| | $m = 1 \cdot 75$. | $m = 3 \cdot 45$. | $m = 7 \cdot 15$. |
|---|---|---|---|
| Maximum | −0·690 | −0·703 | −0·708 |
| Minimum | −0·668 | −0·681 | −0·678 |
| Average | −0·678 | −0·690 | −0·691 |

E.M.F. of Cells set up with Brightened Lead and $m Na_2O, 100H_2O$.

| | $m = 1 \cdot 75$. | $m = 3 \cdot 45$. | $m = 7 \cdot 15$. |
|---|---|---|---|
| Spongy silver (acetate) | 0·921 | 0·928 | 0·954 |
| Spongy palladium | .. | 0·873 | .. |
| Spongy platinum | 0·756 | 0·773 | 0·776 |
| Palladium foil | 0·738 | 0·758 | 0·764 |
| Spongy gold | 0·738 | 0·753 | 0·768 |
| Graphite | 0·721 | 0·738 | 0·738 |
| Gold foil and lead | 0·717 | 0·736 | 0·745 |
| Platinum foil and lead | 0·712 | 0·733 | 0·747 |
| Silver foil and lead | 0·704 | 0·706 | 0·737 |
| Carbon | .. | 0·636 | .. |

Here, as with cells set up with zinc, the E.M.F. rises with the solution-strength, but not to so great an extent, since the (negative) value of $K_2$ also increases therewith.

*Cells set up with Dilute Sulphuric Acid and Copper as Oxidisable Metal.*

As a general rule, cells set up with dilute sulphuric acid showed somewhat less steadiness, and wider limits of fluctuation between the mean values of duplicates, than cells containing caustic soda; but the numbers obtained were sufficiently concordant to show that, *cæteris paribus*, the E.M.F. of a copper-sulphuric acid aëration plate cell increases with the solution-strength, and that practically no difference is noticeable between the mean value obtained with the thinnest leaves and foils of the same metals up to 0·1 mm. in thickness.

Mean E.M.F. of Cells set up with Electro-copper and $mH_2SO_4,100H_2O$.

| | $m$ = 2·5. | $m$ = 10. | Increment with stronger solution. |
|---|---|---|---|
| um sponge covered with plati-ı black | .. | 0·780 | .. |
| um sponge | 0·636 | 0·658 | 0·022 |
| um sponge: thin layer burnished | .. | 0·530 | .. |
| lium sponge | 0·517 | 0·527 | 0·010 |
| ........ | 0·496 | 0·506 | 0·010 |
| um foil and leaf | 0·444 | 0·460 | 0·016 |
| lium foil | 0·447 | 0·458 | 0·011 |
| foil and leaf | 0·445 | 0·458 | 0·013 |
| ite | 0·449 | 0·456 | 0·007 |
| n (A) | .. | 0·464 | .. |
| n (B) | .. | 0·446 | .. |
| y silver | .. | About 0·35* | .. |
| foil and leaf | .. | About 0·25* | .. |

*Set up with Dilute Sulphuric Acid and Amalgamated Zinc, Bright Cadmium, and Silver Foil, as Oxidisable Metals.*

value of $K_2$, the effect of substituting amalgamated zinc for ·, was found to be generally some 2 to 3·5 centivolts higher at ıan after standing awhile; after one or two hours, the zinc were generally more or less coated with minute bubbles of ;en through local action, and then gave pretty constant readings

alue of $K_2$ = effect of substituting other Metals for Copper in $mH_2SO_4,100H_2O$.

| | Zinc. | | Cadmium. | | Silver. |
|---|---|---|---|---|---|
| | $m$ = 2·5. | $m$ = 10. | $m$ = 2·5. | $m$ = 10. | $m$ = 10. |
| num | +1·054 | +0·978 | +0·731 | +0·742 | −0·29 |
| num | +1·033 | +0·962 | +0·705 | +0·709 | −0·12 |
| ge | +1·045 | +0·970 | +0·720 | +0·725 | Abt. −0·20 |

e values obtained with silver aëration plates of all kinds were most irregular and ; in steadiness; with spongy silver (both from acetate and from chloride) rs were obtained varying between 0·28 and 0·43, in the majority of cases not ı *0·34 to 0·36;* and with foil and leaf, numbers lying between 0·17 and

for some hours longer; the following figures refer to this latter period when nearly constant but lower values were obtained. Cadmium did not alter so markedly on standing; silver gave very irregular values.

E.M.F. of Cells set up with Amalgamated Zinc, Brightened Cadmium, and Pure Silver Foil in $m\mathrm{H_2SO_4,100H_2O}$.

| | Zinc. | | Cadmium. | | Silver. |
|---|---|---|---|---|---|
| | $m = 2 \cdot 5$. | $m = 10$. | $m = 2 \cdot 5$. | $m = 10$. | $m = 10$. |
| Platinum sponge coated with platinum black. | .. | 1·750 | .. | 1·505 | About 0·58 |
| Platinum sponge...... | 1·681 | 1·628 | 1·356 | 1·383 | „ 0·46 |
| Palladium sponge..... | 1·562 | 1·497 | 1·237 | 1·252 | „ 0·33 |
| Gold sponge......... | 1·541 | 1·476 | 1·216 | 1·231 | „ 0·31 |
| Platinum foil and leaf. | 1·489 | 1·430 | 1·164 | 1·185 | „ 0·26 |
| Palladium foil........ | 1·492 | 1·428 | 1·167 | 1·183 | „ 0·26 |
| Gold foil and leaf..... | 1·490 | 1·428 | 1·165 | 1·183 | „ 0·26 |
| Graphite............ | 1·494 | 1·426 | 1·169 | 1·181 | „ 0·255 |
| Carbon (mean of A and B)............ | .. | 1·425 | .. | 1·180 | .. |

In the case of the cadmium cells, the E.M.F. rises with the solution-strength as it does in the copper cells, and more rapidly because $K_2$ is positive and also rises; but in the case of the zinc cells, the E.M.F. *falls* as the solution-strength rises, because $K_2$ is here much smaller with the stronger solution.

### *Cells Set up with Ammoniacal Solutions as Electrolytic Fluids, and Electro-copper as Oxidisable Metal.*

It was found impossible to keep any one cell of this kind under anything like constant conditions as regards the nature of the fluid on account of the loss of ammonia, experienced to a large extent even when covered with a jar, &c.; accordingly, the following values can only be regarded as approximate, especially in the case of the stronger solutions. The strength of the fluid was ascertained by sampling and analysis from time to time, and consequently interpolation was sometimes requisite in order to reduce the values obtained with different sets of plates to the same mean strength. On the whole, however, the figures indicate that with solutions of pure ammonia, the E.M.F. rises with the strength of the solution; and similarly with liquids containing sal-ammoniac as well.

M.F. of Cells set up with Electro-copper and Ammoniacal Solutions, $x\mathrm{NH_3}, y\mathrm{NH_4Cl}, 100\mathrm{H_2O}$.

| | $y = 0$ (pure ammonia). | | | $y = 0·8$. | $y = 5$. | |
|---|---|---|---|---|---|---|
| | $x = 1·25$. | $x = 2·0$. | $x = 2·6$. | $x = 2·6$. | $x = 2·6$. | $x = 12$. |
| r platinum. | 0·440 | 0·460 | 0·470 | 0·552 | 0·579 | .. |
| r gold..... | 0·410 | 0·450 | 0·460 | 0·492 | 0·522 | .. |
| r silver.... | 0·395 | 0·410 | 0·420 | .. | 0·471 | .. |
| ι (A)...... | .. | .. | .. | .. | 0·470 | 0·570 |
| ium foil... | . | .. | .. | .. | 0·450 | 0·560 |
| ım foil.... | 0·395 | 0·405 | 0·410 | 0·437 | 0·450 | 0·520 |
| ite........ | 0·330 | 0·340 | 0·355 | 0·404 | 0·445 | 0·510 |
| oil........ | .. | .. | .. | .. | 0·403 | .. |
| .......... | .. | .. | .. | .. | 0·403 | .. |

ı brine saturated with ammonia and spongy platinum aëration higher values still were obtained, reaching up to about 0·75 as imum; but, owing to evaporation of ammonia, these high rapidly diminished on standing a short time.

*t up with Amalgamated Zinc and Bright Silver Foil as Oxidisable Metals.*

following numbers were obtained as the average values for f this kind:—

ıes of $K_2$ = effect of substituting other Metals for Copper in $x\mathrm{NH_3}, y\mathrm{NH_4Cl}, 100\mathrm{H_2O}$.

$x = 2·6$.

| | $y = 0$ (pure ammonia). | $y = 0·8$. | $y = 5$. |
|---|---|---|---|
| amated zinc..... | +0·960 | +0·930 | +0·920 |
| silver.......... | −0·385 | −0·340 | −0·340 |

E.M.F. of Cells set up with Amalgamated Zinc or Bright Si
Ammoniacal Solutions, $x NH_3, y NH_4Cl, 100 H_2O$.

$x = 2{\cdot}6$.

| | Zinc. | | | Silver. | |
|---|---|---|---|---|---|
| | $y = 0$. | $y = 0{\cdot}8$. | $y = 5$. | $y = 0$. | $y = 0{\cdot}8$. |
| Platinum sponge | 1·430 | 1·482 | 1·499 | 0·085 | 0·212 |
| Gold sponge ... | 1·420 | 1·422 | 1·442 | 0·075 | 0·152 |
| Silver sponge... | 1·380 | .. | 1·391 | 0·085 | .. |
| Platinum foil .. | 1·370 | 1·367 | 1·370 | 0·025 | 0·097 |
| Graphite ...... | 1·315 | 1·334 | 1·365 | .. | 0·064 |

In general the E.M.F. rises with increased solution-s
platinum foil being exceptional when opposed to zinc, the E.M
being practically constant.

The various tables above stated clearly show that the
magnitude of the E.M.F. generated when a given kind of
plate is opposed to a given oxidisable metal, depends on the
and to some extent also on the strength, of the solution used as
lytic fluid. The following table indicates the relative order i
various plates come in solutions of caustic soda, sulphuric a
ammoniacal liquors respectively :—

*Caustic Soda.*

- Silver sponge (acetate).
- Palladium sponge.
- Silver sponge (chloride).
- Platinum sponge.
- Platinum black.
- Palladium foil.
- Gold sponge.
- Graphite.
- Gold foil and leaf.
- Platinum foil and leaf.
- Silver foil and leaf.
- Carbon.

*Sulphuric Acid.*

- Platinum black.
- Platinum sponge.
- Palladium sponge.
- Gold sponge.
- Platinum foil and leaf.
- Palladium foil.
- Gold foil and leaf.
- Graphite.
- Carbon.
- Silver sponge.
- Silver foil and leaf.

*Ammoniacal l*

- Platinum spong
- Gold sponge.
- Silver sponge.
- Carbon.
- Palladium foil.
- Platinum foil.
- Graphite.
- Gold foil.
- Silver foil.

In all cases a metal in the state of sponge gives a higher val
when in the state of polished foil or thin leaf.

*Comparison between the E.M.F. generated in Aëration Plate C*
*the Chemical Action going on therein.*

*In the case of* cells with caustic soda as electrolytic fluid,
*chemical change* is the oxidation of the oxidisable metal

oxide (or hydroxide) which in the case of zinc and lead further dissolves in the alkaline liquid forming zincate or plumbate; the heat of solution of zinc and lead oxides in caustic soda being unknown, the total heat development cannot be exactly calculated. According to Julius Thomsen, Zn,O = 85430, and Pb,O = 50300 gram-degrees, corresponding with the E.M.F's. 1·837 and 1·081 respectively,* wherefore the E.M.F. due to the chemical action (including formation of zincate and plumbate) must be higher still; on the other hand, the highest values observed in any aëration cell were only 1·645 and 0·954 respectively (spongy silver-acetate), whilst values of from 1 to 4 decivolts lower still were observed with other plates. Hence *the E.M.F. actually generated in these cells falls very considerably short of that corresponding with the chemical change*, even under the most favourable circumstances, *i.e.*, when producing only an infinitesimal current; whilst when producing a somewhat greater current, but still of only small density (not exceeding a fraction of a micro-ampère per square centimetre of aëration plate surface in some cases), running down and marked depreciation of E.M.F. is rapidly brought about.

Much the same remarks apply to cells set up with sulphuric acid and with ammoniacal fluids; in the former the nett chemical change is the oxidation of the metal and solution of the oxide in the acid forming the sulphate. Julius Thomsen gives the heat values—

| | | | | | |
|---|---|---|---|---|---|
| Zinc...... | $Zn,O,SO_3aq$ | = | 106090 | gram-degrees | = 2·281 volts. |
| Cadmium.. | $Cd,O,SO_3aq$ | = | 89500 | ,, | = 1·924 ,, |
| Copper.... | $Cu,O,SO_3aq$ | = | 55960 | ,, | = 1·203 ,, |
| Silver..... | $Ag_2,O,SO_3aq$ | = | 20390 | ,, | = 0·438 ,, |

Whilst the highest observed values in the case of the first three metals *fall short of these by 4 to 5 decivolts*, and with less active aëration plates the deficiency is much greater. Silver, however, when employed as oxidisable metal, does not show this falling off, but rather the reverse, the highest value observed (platinum black) being about 0·58, and the next highest (platinum sponge) about 0·46, both *exceeding* the E.M.F. calculated from the heat value; obviously this is due, not to anything connected with the aëration plates, but rather to the large negative value of the thermo-voltaic constant† pertaining to silver in contact with sulphuric acid, evidenced also by the circumstance observed by us, that when silver is substituted for zinc in a Grove's cell, instead of the E.M.F. being depressed by an amount

* Taking $J = 41·5 \times 10^6$, and the unit C.G.S. current as evolving 0·0001036 gram of hydrogen per second, whence the factor for converting gram-degrees into volts is sensibly $4800 \times 10^{-8} = 0·000048$ per gram-equivalent.

† 'Phil. Mag.,' vol. 19, 1885, pp. 1 and 102.

corresponding with the difference in heat of formation of zin( silver sulphates (85700 gram-degrees = 1·843 volts) it is depressed by an amount short of this by some 5 or 6 deci Similarly in the ammoniacal cells where (as in the caustic soda the action consists in the oxidation of a metal and the solution ( oxide formed in the ammoniacal liquor, Julius Thomsen give heat values—

| | | |
|---|---|---|
| Zinc .......... | $Zn,O = 85430$ | $= 1·837$ volt. |
| Copper ........ | $Cu_2,O = 40810$ | $= 0·877$ „ |
| Silver.......... | $Ag_2,O = 5900$ | $= 0·127$ „ |

Whence the E.M.F. corresponding with the total chemical cl must somewhat exceed these amounts by the quantity represe: the respective heats of solution in ammonia liquor of the me oxides: the highest values observed with zinc and copper *fall tinctly short of these amounts*, whilst the numbers obtained many kinds of aëration plates in weaker solutions exhibit a deficiency; on the other hand, cells containing silver as oxidi metal show no large falling off, and in the case of the highest v an actual *excess* of E.M.F., again indicating a somewhat large neg value for the thermo-voltaic constant applicable to silver in co with ammoniacal fluids.

It is noticeable that the values of $K_2$ deduced above are not w different from those equivalent to the difference in heat of oxid of the various metals, silver excepted: thus with the caustic cells—

$$\left.\begin{matrix} Zn,O = 85430 \\ Pb,O = 50300 \end{matrix}\right\} \text{Difference } -35130 = -0·755 \text{ volts.}$$

Observed values...... from $-0·678$ to $-0·691$.

With the sulphuric acid cells the differences between the he formation of copper sulphate, and that of zinc, cadmium, and ı sulphates respectively, are $+50130$, $+33540$, and $-35570$, c sponding with—

| | Volts. | Observed values of K |
|---|---|---|
| Copper replaced by zinc | $= +1·078$ | $+0·970$ to $+1·054$ |
| Copper replaced by cadmium | $= +0·721$ | $+0·720$ to $+0·725$ |
| Copper replaced by silver | $= -0·765$ | About $-0·020$ |

With the ammoniacal cells the differences between the hea *formation of* cuprous oxide and of zinc and silver oxides respect *are* $+44620$, and $-34910$, corresponding with—

| | *Volts.* | *Observed values of $K_2$.* |
|---|---|---|
| Copper replaced by zinc | = +0·960 | +0·920 to +0·960 |
| Copper replaced by silver | = −0·750 | −0·340 to −0·385 |

Whilst with zinc, lead, copper, and cadmium, the observed values of $K_2$ in no case differ very widely from those equivalent to the differences in heat of formation, those observed with silver show large differences, indicating as before that silver exhibits a high negative value for its thermo-voltaic constant in each case, viz., −0·5 to −0·6 in contact with dilute sulphuric acid, and near to −0·4 in contact with ammoniacal fluids, this latter value being close to those found previously for silver in contact with neutral solutions of its sulphate, nitrate, and acetate (*loc. cit.*).

On the whole, except when an oxidisable metal is used exhibiting a high negative value for its thermo-voltaic constant, the E.M.F. of a cell containing an aëration plate and an oxidisable metal always falls short, and sometimes largely short, of that equivalent to the chemical changes going on therein even under the most favourable conditions when generating only an infinitesimal current, the deficiency being still more marked when the current density is not so minute: in other words, the *modus operandi* of cells of this class is such as necessarily to render a large fraction of the energy non-adjuvant so far as current is concerned. Just the same remarks apply, as far as our experiments have gone, to cells in which the oxidisable substance is in solution, an extreme case of which is exhibited by cells set up with a solution of sulphurous acid and a submerged platinum foil plate, opposed to an aëration plate of platinum sponge on the surface of dilute sulphuric acid. Such cells give an E.M.F. (when generating only extremely small currents) of from 0·2 to 0·3 volt, whilst the heat of oxidation of sulphurous acid solution, $SO_2aq,O$, is 63634 gram-degrees, according to Julius Thomsen, corresponding with 1·368 volt, or upwards of a volt more than that actually produced.* Analogous diminutions in E.M.F. are brought about in many other cases, to extents depending not only on the nature of the aëration plate but also on that of the oxidisable fluid.

* A large part of the depreciation in this case is due to the fact that sulphurous acid solution and platinum constitute an oxidisable portion of a cell behaving as magnesium and aluminium do in cells where they replace zinc, *i.e.*, giving a much smaller E.M.F. than that due to the heat corresponding with the chemical change: thus, if a cell be set up with zinc or dilute sulphuric acid opposed to platinum in sulphuric chromic acid solution, and the zinc and sulphuric acid be then replaced by platinum and sulphurous acid solution, the E.M.F. falls by an amount greater by 0·45 to 0·5 volt than that corresponding with the differences in heat evolution between $Zn,O,SO_3aq$ and $SO_2aq,O$ (*viz.*, 106090 − 63634 = 42456 gram-degrees = 0·918 volt): *and similarly with other* oxidising fluids. Solutions of alkaline *sulphites behave similarly.*

*Effect of Substituting Oxygen for Air.*

In order to see if any material improvement in the E.M.F aëration cells could be effected by substituting tolerably pure ox for atmospheric air, we carried out a number of observations plates under a bell-jar supplied with purified oxygen from a reser by means of tubes passing through a cork in the narrow mo Readings were first taken for a few days with ordinary air in the oxygen was then admitted and passed through till gradually all was displaced, and after a day or two when the readings had bec constant another series of readings for some days was taken. oxygen was then displaced by air and another series taken, and s alternately several times. The following average values were mately obtained showing a small, though decided, increment E.M.F. when atmospheric air was replaced by oxygen.

Increment in E.M.F. in Oxygen.

| | Caustic soda, $7 \cdot 15Na_2O,100H_2O$. | Sulphuric acid, $10H_2SO_4,100H_2$ |
|---|---|---|
| Platinum sponge | 0·016 | 0·028 |
| Platinum foil | 0·012 | 0·001 |
| Gold sponge | .. | 0·032 |
| Gold foil | 0·012 | 0·002 |
| Palladium sponge | .. | 0·033 |
| Palladium foil | 0·013 | .. |
| Silver sponge | 0·016 | .. |
| Silver foil | 0·016 | .. |
| Graphite | 0·015 | 0·002 |

*Aëration Plates in Contact with Oxidisable Atmospheres.*

Some analogous experiments were made with aëration plat contact with an oxidisable atmosphere (hydrogen or coal-gas) an electrolytic fluid united by means of a siphon with an ext vessel containing an oxidising solution (alkaline permanga sulphuric acid containing chromic acid, nitric acid, &c.) in whi plate of platinum foil was immersed. The readings thus obt were nothing like as concordant as those above described (pro from the difficulty of excluding air completely), showing a tend to rise continually. The following readings were obtained several days when the rise had either ceased or greatly slacken most cases; little difference was observed whether pure hydrog coal-gas was used.

*A. Cells set* up with $7 \cdot 15Na_2O,100H_2O$ in contact with the aër *plates, opposed to* platinum foil immersed in a solution of the

h shaken up with powdered potassium permanganate to ion.

'ells set up with $10H_2SO_4,100H_2O$ in contact with the aëration ›pposed to platinum immersed in the same liquid after agitation ıromic anhydride to saturation.

| | A. Alkaline cells. | | B. Acid cells. | |
|---|---|---|---|---|
| | Hydrogen. | Coal-gas. | Hydrogen. | Coal-gas. |
| ım sponge ..... | 1·525 | 1·10 | 1·02 | 1·10 |
| ım foil ........ | 0·865 | 0·85 | 0·89 | 0·895 |
| sponge ........ | 0·422 | 0·425 | .. | .. |
| foil ........... | 0·73 | 0·78 | .. | .. |
| ponge......... | .. | .. | 0·845 | 0·85 |
| oil............ | 0·72 | 0·75 | 0·87 | 0·90 |
| ium sponge .... | .. | .. | 1·37 | 1·37 |
| ium foil ....... | 0·87 | 0·81 | 0·89 | 1·12 |
| ite ........... | 0·845 | 0·83 | 0·85 | 0·85 |

naking these observations currents were used, the density of in no case exceeded 0·02 micro-ampère per square centimetre of n plate surface.

ıgy platinum and palladium obviously are far more effective as s the E.M.F. set up than the other plates used; the chemical taking place may be regarded as the decomposition of alkaline ıganate into hydrated manganese dioxide, caustic potash, and ı (or of chromic anhydride and sulphuric acid into chromium te, water, and oxygen), and the combination of hydrogen with ;gen thus set free; according to Thomsen's values, the heat ped would accordingly be per 16 grams oxygen evolved—

| | Alkaline cells. | Acid cells. |
|---|---|---|
| position of ising agent.. | $\frac{1}{3} \times 28355 =$ 9452 | $\frac{1}{3} \times 30407 =$ 10136 |
| ;ion of hydrogen ....... | = 68360 | 68360 |
| | 77812 | 78496 |
| ponding with volts...... | = 1·673 | 1·688 |

ce, even with the most effective plates, the E.M.F. actually ted falls distinctly short of that corresponding with the heat of ɜal change. On making the current passing larger by diminish- ɜ external *resistance, the* E.M.F. always fell rapidly; so that in *to obtain a current* capable of producing any considerable

amount of electrolytic decomposition in a voltameter, it was practically impossible to have an acting E.M.F. as high as 1 volt, even with tolerably large platinum sponge plates.

Much the same result was obtained on opposing to one another two platinum sponge aëration plates, one in an atmosphere of hydrogen or coal-gas, the other in contact with air; in no case could any current capable of depositing a few milligrams of silver per day be obtained with an E.M.F. as great as ·1 volt; *i.e.*, a total depreciation of upwards of 0·5 volt was occasioned, or more than one-third of the energy due to the chemical change, viz., oxidation of hydrogen to water, representing 68360 gram-degrees, or 1·470 volt. The economical production of currents by the direct oxidation of combustible gases, therefore, does not seem at present to be a problem likely to be readily solved.

The Society then adjourned over Ascension Day to Thursday, May 17th.

*Presents, May* 3, 1888.

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*May* 17, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Electromotive Properties of the Leaf of *Dionæa* in the Excited and Unexcited State. No. II." By J. BURDON SANDERSON, M.A., M.D., F.R.S., Professor of Physiology in the University of Oxford. Received April 17, 1888.

(Abstract.)

The author has continued his experimental enquiries, of which the results were communicated to the Royal Society under the same title in 1881. In the introduction to the paper he gives a summary of his previous observations, which led to the conclusion that the property, by virtue of which the excitable structures of the leaf respond to stimulation, is of the same nature with that possessed by the similarly-endowed structures of animals. He then proceeds to state that the main purpose of his subsequent investigations has been to determine the relation between two sets of phenomena which might, in accordance with the language commonly used in animal physiology, be termed respectively those of the "resting current" and of the "action

current" of the leaf, *i.e.*, between the electrical properties possessed by the leaf when stimulated, and those which it displays when at rest. Assuming the excitatory response in the leaf to be of the same nature as the excitatory variation or "action current" in muscle and nerve, the question has to be answered, whether in the leaf the response is a sudden diminution of a previously existing electromotive action (according to the pre-existence theory of du Bois-Reymond), or the setting up at the moment of stimulation of a new electromotive action—in short, whether and in how far the two sets of phenomena are interdependent or the contrary.

An observation recorded in his former paper suggested proper methods. It had been shown that by passing a weak voltaic current through the leaf for a short period in a particular direction, its electromotive properties could be permanently *modified* without loss of its excitability. If it could be shown that the influence of this modification extended to both orders of phenomena, those of rest and of excitation, and that both underwent corresponding changes of character under similar conditions, this would go far to prove that an essential relation existed between them.

Acting on this suggestion, the author has had recourse to modes of experiment similar to those which have been employed during the last few years in the investigation of the newly-discovered "secondary electromotive" phenomena of muscle and nerve (see 'Oxford Biological Memoirs,' vol. 1, part 2). The details of these experiments, made in 1885, are given in the first three sections of the paper. They relate to (1) the more immediate effect of the current as seen in the records of successive galvanometric observations made at regular intervals; (2) the more permanent influence of the current on the electromotive properties of the unexcited leaf, and on its electrical resistance; and (3) the concomitant modification of its behaviour when stimulated.

The general result of these experiments is to show that the two orders of phenomena, the excitatory and those which relate to the resting state, are so linked together that every change in the state of the leaf when at rest conditionates a corresponding change in the way in which it reacts to stimulation—the correspondence consisting in this, that the direction of the response is opposed to that of the previous difference of potential between the opposite surfaces, so that as the latter changes from ascending to descending, the former changes from descending to ascending.

The author considers that this can only be understood to mean that the constantly operative electromotive forces which find their expression in the persistent difference of potential between the opposite surfaces, and those more transitory ones which are called into momentary existence by touching the sensitive filaments or by

other modes of stimulation, have the same seat, and that the opposition between them is in accordance with a principle applicable in common to the excitable structures of plants and animals, viz., that the *property which renders a structure capable of undergoing excitatory change is expressed by relative positivity, the condition of discharge by relative negativity.*

With reference to the mode of action of the voltaic current, the effect produced in the unexcited leaf is compared with that observed in the unexcited electric organ of the skate or the torpedo, in both of which, as in the leaf, it is observed that, although the after-effect of a current led across the disks or plates is to increase the difference of potential between its two surfaces, whichever way the current is directed, the effect is much greater when the direction of the external current coincides with that of the normal electromotive action of the organ than in the opposite case.

It is further shown that the electromotive changes concerned in "modification" and "excitation" have their seat at the upper surface of the lamina. If, as the author believes, all these changes depend on difference of physiological activity between adjacent excitable cells or strata of cells of which the protoplasmic linings are in continuity, it must be supposed that when the leaf is at its prime, the most superficial strata are positive to those subjacent, and that as the former lose their pristine susceptibility of excitatory change, the physiological, and consequently the electrical, difference between them is diminished, annulled, or reversed.

The fourth section of the paper is devoted to an investigation made in 1887, of the events of the first second after excitation made with the aid of a pendulum-rheotome specially adapted for the purpose. The fifth contains the description of the records obtained by photographing the electric phenomena of the excitatory reaction, as observed with the aid of the capillary electrometer, on rapidly moving plates. Both of these series of observations serve to confirm and complete the results obtained by other methods.

## II. "Magnetic Qualities of Nickel." By J. A. Ewing, F.R.S., Professor of Engineering, University College, Dundee, and G. C. Cowan. Received April 26, 1888.

(Abstract.)

The experiments described in the paper were made with the view of extending to nickel the same lines of enquiry as had been pursued by one of the authors in regard to iron ('Phil. Trans.,' 1885, p. 523). *Cyclic processes* of magnetisation were studied, in which a magnetising

force of about 100 c.g.s. units was applied, removed, reversed, again removed, and re-applied, for the purpose of determining the form of the magnetisation curve, the magnetic susceptibility, the ratio of residual to induced magnetism, and the energy dissipated in consequence of hysteresis in the relation of magnetic induction to magnetising force. Curves are given, to show the character of such cycles for nickel wire in three conditions: the original hard-drawn state, annealed, and hardened by stretching after being annealed. The effects of these have also been examined (1) by loading and unloading magnetised nickel wire with weights which produced cyclic variations of longitudinal pull, and (2) by magnetising while the wire was subjected to a steady pull of greater or less amount. The results confirm and extend Sir William Thomson's observation that longitudinal pull diminishes magnetism in nickel. This diminution is surprisingly great: it occurs with respect to the induced magnetism under both large and small magnetic forces, and also with respect to residual magnetism. The effects of stress are much less complex than in iron, and cyclic variations of stress are attended by much less hysteresis. Curves are given to show the induced and residual magnetism produced by various magnetic forces when the metal was maintained in one or other of certain assigned states of stress; also the variations of induced and residual magnetism which were caused by loading and unloading without alteration of the magnetic field. Values of the initial magnetic susceptibility, for very feeble magnetising forces, are stated, and are compared with the values determined by Lord Rayleigh for iron, and the relation of the initial susceptibility to the stress present is investigated. The paper consists mainly of diagrams in which the results are graphically exhibited by means of curves.

III. "On the present Position of the Question of the Sources of the Nitrogen of Vegetation, with some new Results, and preliminary Notice of new Lines of Investigation." By Sir J. B. LAWES, F.R.S., and J. H. GILBERT, M.A., LL.D., F.R.S., Sibthorpian Professor of Rural Economy in the University of Oxford. Received, Part I, July 20, 1887. Parts II and III, May 3, 1888.

[For Preliminary Notice of this Paper, see vol. 43, p. 108.]

IV. "On the Rhythm of the Mammalian Heart." By J. A. McWilliam, M.D., Professor of the Institutes of Medicine in the University of Aberdeen. Communicated by Professor M. Foster, Sec. R.S. Received April 26, 1888.

The following are some of the general conclusions arrived at from a prolonged investigation of the rhythm of the mammalian heart. The experiments were conducted on the cat, dog, rabbit, rat, hedgehog, and guinea-pig, the cat being the animal most commonly used. The animals were anæsthetized, artificial respiration was kept up, the thorax was laid open, and the action of the heart was recorded by various adaptations of the graphic method:—

1. Minimal stimulation of the quiescent cardiac muscle is at the same time maximal; a stimulus which is strong enough to excite contraction at all excites a maximal contraction. The strength of an artificially excited beat does not depend on the strength of the stimulus; it is equally strong with maximal and minimal excitation. I have tested this point in various ways:—

(1.) On the excised heart which has ceased contracting spontaneously, but is still quite capable of being artificially excited to beat.

(2.) On the intact heart reduced to a state of quiescence by vagus stimulation.

(3.) On intact hearts which beat slowly in consequence of cooling and other circumstances; the stimulations were applied during the quiescent period intervening between two spontaneous contractions.

2. The condition of fibrillar contraction or heart-delirium induced in the ventricles of excitable hearts by the application of interrupted currents and other means can be recovered from even after long periods (three-quarters of an hour, &c.) under the combined influence of artificial respiration, rhythmical compression of the ventricles, and the administration of pilocarpin.

When the excitability of the cardiac muscle has been much depressed (by pilocarpin, certain phases of exhaustion, &c.), the application of interrupted currents does not induce fibrillar contraction, but merely a series of rhythmic beats in the case of a quiescent organ, or an acceleration of the rhythm already present in a heart which is beating spontaneously.

3. The spontaneous rhythmic power possessed by the terminal parts of the great veins, the auricles, and the ventricles, seems, *in some conditions at least,* to be myogenic.

4. *In the intact* heart the auricles and ventricles do not beat in virtue of their own independent rhythmic power, but in obedience to

mpulses reaching them from the terminal or "ostial" parts of the reat veins. For though both auricles and ventricles possess an nherent rhythmic tendency, the ostial parts of the great veins possess higher power of spontaneous rhythm, and hence dominate the hythm of the whole heart. The rapidly recurring contractions rising in the ostial regions are propagated over the whole organ; the nore rapid rhythm of the ostial parts supersedes and renders latent he less rapid inherent rhythm of the auricles and ventricles. In upport of this view there can be adduced many facts. Among thers—

(1.) The independent rhythm of the auricles and ventricles appears be decidedly slower than that of the terminal or ostial parts of the eins.

(2.) Slight heating of the ostial part of a great vein (*e.g.*, the ermination of the vena cava superior) causes a marked acceleration n the rhythm of the whole heart, while a similar heating of the entricular wall causes very little change, or (more commonly) none t all.

Weak faradic and galvanic currents induce similar results in this espect.

(3.) In the dying heart the power of spontaneous rhythmic contraction survives longest in the ostial parts of the veins. This is nalogous to what obtains in the hearts of cold-blooded animals, where he greatest vitality is exhibited by the sinus venosus, the part possessed of the highest spontaneous rhythm, *i.e.*, the leading or dominant art of the organ.

5. The normal sequence of the ventricular contraction upon the uricular contraction in the intact heart is essentially determined by ervous influences. It is not dependent on—

(1.) The distension of the ventricles with blood pumped in from he auricles.

(2.) The mechanical relations normally obtaining between the uricles and ventricles through the medium of the auriculo-ventricular alves and the chordæ tendineæ; or

(3.) The occurrence of an electrical change (current of action) in he auricular muscle as one of the phenomena of its contraction.

6. The nervous influence determining the ventricular sequence is robably of an intermittent character.

7. The propagation of the contraction *within* the walls of the uricles and ventricles is not dependent on the action of the nerves lying near the surface of these parts.

The contraction continues to be propagated quite well when the surface (*e.g.*, of the ventricles) has been washed with strong ammonia.

8. *In the auricles at least*, the ordinary beat is not the result of a

simultaneous motor discharge from a nerve centre to all the muscular fibres; the contraction is, on the other hand, a progressive process passing over the auricular walls in a wave-like fashion.

9. A reversal of the normal sequence of the heart's contraction can be induced and kept up for a considerable time by applying to the ventricles a series of single stimulations (*e.g.*, induction shocks) at a rate somewhat more rapid than that of the spontaneous rhythm of the organ.

## V. "Inhibition of the Mammalian Heart." By JOHN A. MCWILLIAM, M.D., Professor of the Institutes of Medicine in the University of Aberdeen. Communicated by Professor M. FOSTER, Sec. R.S. Received May 3, 1888.

The following conclusions are based upon a long series of experiments performed upon the cat, dog, rabbit, rat, hedgehog, and guinea-pig, the cat being the animal commonly used. The animals were anæsthetized, usually with chloroform; artificial respiration was kept up; the thorax and often the pericardial sac were laid open, and the action of the heart was examined with the aid of the graphic method.

### *Section of the Vagi.*

The results of section of both vagi vary according to the conditions obtaining at the time the nerves are cut—according to the amount of controlling influence exercised by the medullary cardio-inhibitory centre upon the heart. When the cardio-inhibitory centre is inactive, section of the vagi causes no appreciable change in the heart's action. On the other hand, section of the nerves at a time when the controlling influence of the medullary centre is acting to a decided extent, is followed by very pronounced results—by an increase not only in the rate of the cardiac beat, but also in the contraction force of both the auricles and the ventricles. There is a marked augmentation in the strength of the beats; the change in the energy of the auricular contractions is usually *more* extensive than that occurring in the case of the ventricles.

### *Stimulation of the Vagus Nerve.*

The latent period of vagus stimulation varies remarkably in different conditions; there is often a period of many seconds before the heart stands still.

When the vagus nerve is stimulated so as to *slow* the heart, it is *usually seen that* the inhibitory influence is not of maximal intensity *at its first* manifestation, but goes on increasing for some time.

*Effects of Vagus Stimulation on the Auricles.*

1. The vagus appears as a rule to influence the auricles more adily and more powerfully than the ventricles.

2. Vagus stimulation leads to a slowing or an arrest of the rhythmic at, and a very marked weakening of the contraction force.

The recommencing auricular beats that occur when the period of hibition is passing away are very weak; and any contractions cited by direct stimulation (*e.g.*, with induction shocks) during the riod of standstill are strikingly enfeebled.

3. Vagus stimulation causes a pronounced depression of the excita-ity of the auricular tissue to direct stimulation.

During the period of inhibition resulting from vagus stimulation it much more difficult than usual to excite an auricular beat by direct citation; a much stronger stimulus is necessary to elicit any con-ction at all.

4. The tone of the auricular muscle appears to be markedly dimi-ihed.

5. These results occur when the vagus is stimulated, even when the perior and inferior venæ cavæ have been clamped, so that the vities of the heart are no longer filled with blood.

6. The vagus nerve seems to exert a powerful influence of a more less direct nature on the muscle itself, not merely by inhibiting or akening the motor impulses which are commonly assumed to pass om nerve centres in the heart to the muscular fibres. For if it were ie that the vagus acted simply by depressing the motor centres of e heart, it is very difficult to conceive how the responsiveness of the ricular muscle to direct stimuli should be so greatly diminished, d how the contraction force should be so strikingly reduced when e auricular muscle is made to contract by induction shocks applied the auricular tissue.

It would seem that whatever changes the vagus may induce in the rve-cells and ganglia occurring plentifully in the auricles, it can so exert an important influence on the contractile tissue itself.

7. Upon the whole, the influence of the vagus nerve upon the mam-ilian auricles presents a close parallelism to what holds good in the ricles of many cold-blooded animals.

*Effects of Vagus Stimulation on the Ventricles.*

Besides causing slowness or standstill, the vagus can cause other iportant changes in the ventricular part of the heart.

1. The contraction force is markedly diminished. When a period of indstill has ended, the recommencing beats are usually weak; and ats *excited by direct stimulation* (*e.g.*, single induction shocks) *ring the period of standstill* are of diminished size.

When vagus stimulation does not cause complete standstill, but only a marked slowing, the strength of the slow ventricular beats is usually much less than the normal.

The reduction in contraction force does not bear any *constant* relation to the degree of slowing. While *all* the slow beats are weakened in *some degree*, a beat occurring after a long pause is *sometimes* decidedly *stronger* than one occurring after a shorter pause; on the other hand, the converse more often holds good—a beat occurring after a long pause is *weaker* than a beat occurring after a shorter pause.

The depression of contraction force does not appear to depend on over-distension of the ventricles during the slowing or standstill; nor upon the fall of arterial pressure that occurs and involves a diminished resistance to the ventricular systole and a change in the coronary circulation.

The force-depressing effects of vagus stimulation can still be seen (1) when the superior and inferior venæ cavæ have been clamped; or (2) when the pulmonary artery or (3) the aorta has been clamped; or (4) when *all* these vessels have been clamped before the vagus stimulation.

2. When slowing or arrest of the ventricular action occurs as a result of vagus stimulation, there is a marked change in the shape and duration of the ventricular curves; the degree of change stands in close relation to the length of the pause preceding each beat. The curves become broader near the top, and their duration is increased. The longer the interval preceding a curve the broader the curve is, and the more markedly is it prolonged. These features are not abolished when the superior and inferior venæ cavæ have been clamped before the vagus stimulation; nor when the aorta or the pulmonary artery, or all these vessels, have been clamped.

3. The vagus appears to inhibit the spontaneous rhythmic tendency inherent in the ventricles; the ventricular standstill does not appear to be due simply to the standstill of the rest of the heart.

4. At the same time the absence of auricular beats of any considerable strength is *usually* a necessary condition for the occurrence of a protracted ventricular standstill. It commonly but not invariably happens that if the auricles are artificially excited to contract during the period of cardiac standstill, the ventricles beat also in sequence to the artificially excited auricular contraction.

5. When the heart begins to beat after a period of inhibition, the order of contraction most commonly seen is that which obtains normally—ostial parts of the great veins; auricles; ventricles. But sometimes the ventricles recommence, and give one or more beats *before* any contraction occurs in the other parts of the heart.

6. There are sometimes seen evidences of the occurrence under vagus influence of a block in the propagation of the contraction from

auricles to ventricles. At certain phases of vagus stimulation the ventricles often fail to respond to auricular beats, while at the same time there is evidence to show that this is due not to a depression of the ventricular excitability, but to a break in the transmission of the contraction from the auricles.

7. The *maximum* intensity of the inhibitory influence exerted by vagus stimulation often obtains at the same time in the auricles and the ventricles. But frequently the auricles become greatly depressed, while the ventricular beats are of undiminished size, or are only beginning to be affected; in rare cases the ventricular contraction force becomes reduced more suddenly than the auricular.

8. The effects of vagus stimulation on the ventricles may be in some measure counteracted by the application to the ventricular surface of a series of stimulations (*e.g.*, single induction shocks) at about the normal rate of the heart's action. An artificially excited series of beats is thus caused; these beats give curves of approximately normal form and duration, and they are much stronger than any slowly occurring spontaneous beats that appear after the standstill has lasted for some time; they are also much stronger than single beats excited (by induction shocks) at long intervals during the standstill. The beats of the artificially excited series (at normal rate) are still decidedly weaker than normal beats.

### *On the Existence of a Local "Inhibitory Area" in the Heart.*

By stimulation of a certain locality on the dorsal aspect of the auricular surface, certain striking effects are obtained. In the cat and dog the area in question is elongated in shape, and is situated over the inter-auricular septum, its long axis running parallel with the plane of the septum. It extends downwards to within a short distance of the coronary sinus. At the right side of the area lies the termination of the vena cava inferior.

Many nerves course downwards through this region; there are also numerous nerve-cells and ganglia. These, however, are not confined to the area in question, but occur in considerable number over the dorsal aspect of the left ventricle, especially in its septal half. The nerves appear to be derived to a considerable extent from the left vagus. The majority of the fibres are non-medullated, but medullated fibres are also present (cat). Ganglia occur in special abundance near the auriculo-ventricular groove.

Stimulation of this area with an interrupted current gives results that stand out in sharp contrast to those obtained by stimulating other parts of the auricular wall, *e.g.*, the appendix. Stimulation of the latter causes an acceleration of both auricles and ventricles. The auricles contract with great rapidity, so that they present a peculiar

fluttering appearance; the ventricles beat much more rapidly than before, though they do not keep pace with the auricles.

On the other hand, stimulation of the inhibitory area, while it causes a rapid fluttering action of the auricles, induces either a very marked slowing, or a complete standstill in the ventricles. This result is a mixed one—ventricular inhibition, resulting from stimulation of certain structures in the inhibitory area, and auricular acceleration, in all probability due to an escape of the stimulating current to the excitable auricular tissue.

The inhibitory effects on the ventricle much resemble those caused by vagus stimulation. There is depression of the ventricular contraction force, and changes in the shape and duration of the ventricular curves similar to those occurring under vagus influence. Stimulation of the inhibitory area and of the vagus are both rendered ineffective by the administration of atropine.

But there are certain points of difference :—

(1.) The strength of current necessary to inhibit the ventricles is very much less when the current is applied to the inhibitory area than when it is applied to the vagus.

(2.) Stimulation of the inhibitory area remains effective in arresting the ventricular action, after curare has been administered in such amount as to cause stimulation of one or both vagi in the neck to be entirely without inhibitory result.

(3.) In many instances when the vagi have become exhausted, or have lost their inhibitory power from less definite causes, the inhibitory area remains effective.

It seems clear from the very different relation borne by the inhibitory area to certain poisons, to the strength of stimulating current necessary, to exhaustion, &c., that in exciting this area we are dealing with structures of a more or less special nature, differing markedly in their character from the ordinary inhibitory fibres running in the trunks of the vagus nerves.

The important structures of the inhibitory area are situated superficially; they may be readily paralysed by the application of a few drops of a 4 per cent. solution of cocaine hydrochlorate, or of strong ammonia.

The region in question does not contain a motor centre for the heart muscle. Destruction of this area does not arrest the spontaneous rhythm of the organ (which indeed originates in parts some distance removed from the inhibitory area, viz., in the ostial parts of the great veins, especially the vena cava superior and the pulmonary veins). Nor is the propagation of the contraction from one part of the heart to another in any way deranged or interfered with.

The inhibitory area probably contains structures to which many at

least of the inhibitory fibres of the vagus go, there to come into intimate relation with the cardiac mechanism.

*Effect of Stimulation of Ostial Parts of Great Veins in certain Abnormal Conditions.*

At certain stages of the process of asphyxia, and in the dying heart, there is often seen a very remarkable alteration in the behaviour of the ostial parts of the great veins towards direct stimulation with interrupted currents. In such circumstances, an inhibition of the spontaneous rhythmic action of these parts may often be seen as a result of direct stimulation, whereas in the normal state such a stimulation is productive of immediate and striking acceleration.

## VI. "On the Structure of the Electric Organ of *Raia circularis*." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Professor J. BURDON SANDERSON, F.R.S. Received April 30, 1888.


(Abstract.)

This paper gives an account of the structure of the cup-shaped bodies, which, as mentioned in a previous paper read 26th April, 1888, make up the electric organs of certain members of the skate family. The structure of these electric cups has been already studied in three species of skate, viz.: *Raia fullonia*, *R. radiata*, and *R. circularis*. The present paper only deals with the electric organ of *R. circularis*. It shows that the cups in this species are large, well-defined bodies, each resembling somewhat the cup of the familiar "cup and ball." The cup proper, like the disks of *R. batis*, consists of three distinct layers, (1) the lining, which is almost identical with the electric plate of *R. batis*; (2) a thick median striated layer; and (3) an outer or cortical layer. The lining or electric plate is inseparably connected with the terminal branches of the numerous nerve-fibres, which, entering by the wide mouth in front, all but fill the entire cavity of the cup, and ramify over its inner surface, the intervening spaces being occupied by gelatinous tissue. This electric layer, which is richly nucleated, presents nearly as large a surface for the terminations of the electric nerves as the electric plate which covers the disk in *R. batis* and *R. clavata*. The striated layer, as in *R. batis*, consists of numerous lamellæ, which have an extremely contorted appearance, but it differs from the corresponding layer in *R. batis*, in retaining a few corpuscles. The cortical layer very decidedly differs in appearance from the alveolar layer in *R. batis*. It is of considerable thickness, contains large nuclei,

and sometimes has short blunt processes projecting from its outer surface. These short processes apparently correspond to the long complex projections which in *R. batis* give rise to an irregular network, and they seem to indicate that the cortical layer of *R. circularis* essentially agrees with the alveolar layer of *R. batis*, differing chiefly in the amount of complexity. Surrounding the cortex there is a thin layer of gelatinous tissue in which capillaries ramify. This tissue evidently represents the thick gelatinous cushion which lies behind the disk in *R. batis*, and fills up the alveoli.

The stem of the cup is usually, if not always, longer than the diameter of the cup. It consists of a core of altered muscular substance, which is surrounded by a thick layer of nucleated protoplasm continuous with the cortical layer of the cup, and apparently also identical with it.

The cups are arranged in oblique rows to form a long, slightly-flattened spindle, which occupies the posterior two-thirds of the tail, being in a skate measuring 27 inches from tip to tip, slightly over 8 inches in length, and nearly a quarter of an inch in width at the widest central portion, but only about 2 lines in thickness.

The posterior three-fifths of the organ lies immediately beneath the skin, and has in contact with its outer surface the nerve of the lateral line. The anterior two-fifths is surrounded by fibres of the outer caudal muscles. It is pointed out that while the organ in *R. circularis* is larger than in *R. radiata*, it is relatively very much smaller than the organ of *R. batis*.

## VII. "On Æolotropic Elastic Solids." By C. CHREE, M.A., Fellow of King's College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 1, 1888.

(Abstract.)

This paper treats of elastic solids of various non-isotropic kinds. Its object is to obtain solutions of the internal equations in ascending integral powers of the variables, and apply them to problems of a practical kind, some of them already solved, but in an entirely different way, by Saint-Venant.

On the multi-constant theory of elasticity the equations connecting the strains and stresses contain 21 constants. As shown by Saint-Venant these reduce for one-plane symmetry to 13, for three-plane symmetry to 9, and for symmetry round an axis perpendicular to a plane of symmetry to 5.

*Part I of this* paper deals with one-plane symmetry. A solution is *obtained of the* internal equations of equilibrium complete so far

it goes. It is employed in solving the problem, already treated by Saint-Venant, of a beam, whose length is perpendicular to the plane of symmetry, held at one end, and at the other acted on by a system of forces, whose resultant consists of a single force along the axis of the beam, and of a couple about any line in the terminal section through its centroid. The cross-section may be any whatever, including the case of a hollow beam, provided it be uniform throughout. The case when the cross-section is elliptical, and the beam exposed to equilibrating torsional couples over its ends is also treated. Results are obtained confirmatory of Saint-Venant's. They are also extended to the case of a composite cylinder, formed of shells of different materials whose cross-sections are bounded by concentric similar and similarly situated ellipses, the law of variation being the same for all the elastic constants of the solution. The limiting case of a continuously varying structure is deduced.

It is found when a beam is exposed to terminal traction, whether uniform or not, that the strain consists in part of a shear in the plane of the cross-section which is proportional to the traction; and the position of the lines in the cross-section, which being originally at right angles remain so, is determined. These lines are called *principal axes of traction*. If there are in addition two planes of symmetry through the axis of the beam, these *principal axes* are the intersections of the planes of symmetry with the cross-section.

When a beam of circular section is exposed to torsion, it is proved that warping will ensue proportional to the moment of the twisting couple. Only two diameters in the cross-section, and these mutually at right angles, remain perpendicular to the axis of the beam. These are called *principal axes of torsion*. If $w$ denote displacement parallel to the axis of the beam, and $r$, $\phi$ denote the undisturbed polar co-ordinates of a point in the cross-section, referred to its centre as origin, and one of these axes as initial line, the law of warping is given by—

$$w \propto r^2 \sin 2\phi.$$

There is in general no connexion between the positions of the principal axes of traction and of torsion, as the expressions giving their inclination to the axes of co-ordinates contain wholly different elastic constants; but for three-plane symmetry of the kind already mentioned they coincide. When the material is symmetrical round the axis of the beam, the shear and the warping of course are found to vanish. It is pointed out how by means of these various properties the nature of the material may be investigated experimentally.

Part II treats of a material symmetrical round an axis, that of $z$, and having the perpendicular plane one of symmetry. A general solution of the internal equations of equilibrium is obtained, sup-

posing no bodily forces to act. The solution involves arbitrary constants, and consists of a series of parts, each composed of a series of terms involving homogeneous products of the variables, such as $x^l y^m z^{n-l-m}$, where $l$, $m$, $n$ are integers, and $n$ is greater than 3. The case $n = 7$ is worked out numerically as an illustration. The terms involving powers of the variables, the sum of whose indices is less than 4, are then obtained by a more elementary process, and these alone are required in the applications which follow. These terms arrange themselves in groups *associated* with certain constants in the expression found for the dilatation.

The first application of the solution is to "Saint-Venant's problem" for a beam of elliptical cross-section. The problem is worked out without introducing any assumptions, and a solution obtained, which is thus directly proved to be the only solution possible if powers of the variables above the third be neglected. Certain groups of *associated constants* vanish completely, and the remaining arbitrary constants express themselves very simply in terms of the terminal forces, all the constants of one group depending on one only of the components of the system of forces.

Part III consists of an application of the second portion of the solution of Part II to the case of a spheroid, oblate or prolate, and of any eccentricity, rotating with uniform angular velocity round its axis of symmetry, $oz$, which is also the axis of symmetry of the material. The surface of the spheroid is supposed free of all forces.

The terms depending on two only of the groups of associated constants suffice, along with a particular solution on account of the existence of what is equivalent to the occurrence of bodily forces, to satisfy all the conditions of the problem, and the strains are determined explicitly.

The limiting form of the solution when the polar axis of the spheroid is supposed to diminish indefinitely, while the equatorial remains unchanged, is applied to the case of a thin circular disk rotating freely about a perpendicular to its plane through its centre. The solution so obtained is shown to satisfy all the conditions required for the circular disk, except that it brings in small tangential surface stresses depending on terms of the order of the thickness of the disk. According to this solution the disk increases in radius, and diminishes everywhere in thickness, especially near the axis, so as to become biconcave. All, originally plane, sections parallel to the faces become very approximately paraboloids of revolution, the latus rectum of each varying inversely as the square of the angular velocity into the original distance of the section considered from the central section.

*Again, by* supposing the ratio of the polar to the equatorial *diameter of the* spheroid to become very great, a surface is obtained *which near the* central plane, $z = 0$, of the spheroid differs very li

of a right circular cylinder. The corresponding form of on obtained for the spheroid, when the ratio of the polar to orial diameter becomes infinite, may thus be expected to y approximately to the portions of a rotating cylinder not the ends, and thus for a long thin cylinder to be for all purposes satisfactory. This is verified directly, and it is at this solution is in all respects as approximately true as ersally accepted for Saint-Venant's problem. According to on the cylinder shortens, and every cross-section increases but remains plane. The shortening and the increase in the e, of course, proportional to the square of the angular

treats of the longitudinal vibrations of a bar of uniform ection and of material the same as in Part II. Assuming the form—

$$\text{radial} = r\,\psi(r)\cos(pz-\alpha)\cos kt,$$

$$\text{longitudinal} = \phi(r)\sin(pz-\alpha)\cos kt,$$

und in terms of $\psi(r)$ by means of the equations established [. From these equations is deduced a differential equation ırth order for $\psi(r)$, and for this a solution is obtained connly positive integral even powers of $r$. A relation exists ng all the constants of the solution in terms of the co- $a_0$ and $a_2$ of $r^0$ and $r^2$. In applying this solution to the mentioned, terms containing powers of $r$ above the fourth :ted, and it is shown to what extent the results obtained are ate.

curved surface the two conditions that the normal and l stresses must vanish determine $a_2$ in terms of $a_0$, and lead lowing relation between $k$ and $p$—

$$k = p\left(\frac{M}{\rho}\right)^{\frac{1}{2}}\{1-\tfrac{1}{4}p^2a^2\sigma^2\}.$$

denotes the density and $a$ the radius of the beam, while M 's modulus, and $\sigma$ the ratio of lateral contraction to longiкpansion for terminal traction. This agrees with a result by Lord Rayleigh* on a special hypothesis.

ling to the terminal conditions, it is shown how $p$ is deterım the conditions as to the longitudinal motion at the ends her quite free or entirely non-existent. Since $a_0$ depends ıe amplitude of the vibrations, we are left with no arbitrary undetermined. If the bar be so "fixed" at its ends, that l motion is *unobstructed*, this leads to no difficulty, but if an

* '*Theory of Sound*,' vol. 1, § 157.

end be "free" a difficulty arises. At such an end the solution requires the existence of a radial stress $U \propto (2i+1)^3 r (a^2 - r^2)/l^3$, where $i$ is an integer depending on the number of the harmonic of the fundamental note and $l$ denotes the length of the bar. The value given above for $k$ thus answers to a problem differing to a certain extent from that occurring in nature in the case either of "fixed-free" or of "free-free" vibrations. There will thus be a difference in these cases between the results of experiment and those of the accepted theory, even as amended by Lord Rayleigh. This divergence will increase rapidly with the order of the harmonic, and though very small for a long thin bar will increase rapidly as the ratio of the diameter to the length is increased. Since in dealing with the conditions at the curved surface, terms of the order $(a/l)^5$ were neglected, the same remarks apply, though to a smaller extent, in the case of the "fixed-fixed" vibrations.

From the values of $u$ and $w$, which are obtained explicitly, it is shown that the hypothesis made by Lord Rayleigh is true as a first, and only as a first, approximation.

The Society adjourned over the Whitsuntide Recess to Thursday, May 31st.

*Presents, May* 17, 1888.

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Three Autograph Letters of Sir Joseph Banks, P.R.S. Mr. J. W. L. Glaisher, F.R.S.

*May* 28, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Croonian Lecture—"Ueber die Entstehung der Vitalen Bewegung"—was delivered by Professor W. Kühne, of Heidelberg, in the Theatre of the Royal Institution.

[Publication deferred.]

*May* 31, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Mr. George King (elected 1887) was admitted into the Society.

Pursuant to notice, Professors Edmond Becquerel, Hermann Kopp, Eduard F. W. Pflüger, and Julius Sachs were balloted for and elected Foreign Members of the Society.

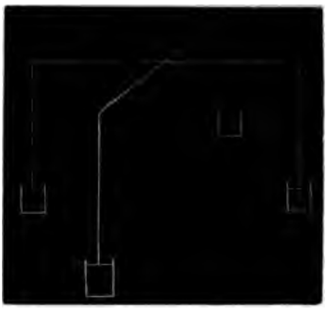
The following Papers were read:—

I. "On the Effect of Occluded Gases on the Thermo-electric Properties of Bodies, and on their Resistances; also on the Thermo-electric and other Properties of Graphite and Carbon." By JAMES MONCKMAN, D.Sc. Communicated by Professor J. J. THOMSON, F.R.S. Received May 1, 1888.

"Le Roux has shown that when a notch is filed into a wire and one side heated there is in general a thermo-electric current. He also found that when two wires of the same metal, with flat ends, are

together, so that one forms a continuation of the other, and e on one side of the junction is heated, no current is obtained, observed a current in all cases where there was dyssymmetry." epeating these experiments, I was led to commence a research effect of occluded gas by the following curious phenomenon. eces of platinum wire of 0·9 mm. section, and of 925 mm. length, retched with weights only just heavy enough to keep them t. They were placed at right angles to each other, the centres n contact, and the ends bending down into mercury cups (see Each wire after being carefully annealed was joined up to a

Fig. 1.



meter, and the absence of currents from strain proved by with a small flame. When both wires were found to be y free, they were brought together in the middle, and one end connected with the galvanometer. On heating the wires near it of contact thermo-electric currents were produced, but after the junction of the wires to a bright red for a little time and g it to cool, the currents produced by heating the wires on de were opposite in direction to those produced before. After from Saturday until Monday the change in the wires, produced ing the point of contact, was found to have disappeared, and rents produced by heating the wires to be the same as at

naturally suggesting that some kind of temporary change took the wire, when heated in a Bunsen lamp, and that this might be produced by the gas absorbed by the platinum at a high ture, I was induced to commence a series of experiments on ct of occluded gases on the electrical properties of bodies. of platinum wire about 18 inches long was bent in the and *one-half protected* by being covered with glass tube and *ter-tight at the lower end.* After annealing the free portion *ig until perfectly* free from all strain effects, it was placed,

FIG. 2.

up to about the middle, in acidulated water, and made the ne pole of a battery, and hydrogen liberated upon it for a few mi After being dried it was tested with a small flame at distar 1 cm. along its whole length. The result was a current fro free wire towards that part on which hydrogen had been pro greatest at the junction of the free wire and the sat wire.

| | H. | | | | | Free wire. | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| The deflections were...... | 0. | 0. | 0. | 7. | 10. | 10. | 7. | 4. | 1. | |
| Another experiment gave.. | 0. 0. | 5. | 5. | 5. | 8. | 8. | 5. | 5. | 0. | ( |

When wires of palladium were used more powerful effects same kind were produced. Thus when two wires were used electrodes in decomposing acidulated water, dried and gently in contact, a current towards the hydrogen was observed. If by a Bunsen flame complications arose from the hydrogen in tl taking fire. The flame produced could easily be seen 4 or 5 mm from the Bunsen flame.

Carbon rods were next tried. Gas-carbon was first tried, but unable to get two rods sufficiently similar in composition to be their own thermo-electric currents being large enough to co changes produced by gases. I had, however, no difficulty in g rods made for arc lamps to answer my purpose. They were hea a red heat to expel gases, and the ends were filed flat.

It was found that when one of these rods was heated and against the other (see fig. 3), the current was always from cold below 200° C.

*They were then* used as the electrodes in decomposing *sulphuric acid*, dried carefully until no current was produced on

[redacted]



FIG. 3.

in contact. On heating either rod and joining them as before, rrent was produced from hydrogen to oxygen across the hot ion.

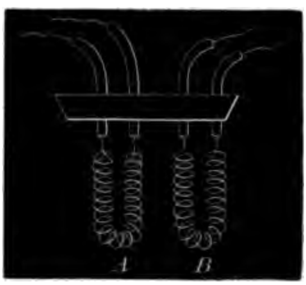
e same effect was obtained by decomposing hydrochloric acid ion, in which case we get chlorine instead of oxygen, and the nt flows from hydrogen and chlorine.

the rod be saturated with $SO_2$ it is found to act like those ining oxygen or chlorine.

*sistance.*—In the first experiments made to try if any change of ance took place when wires are saturated with gas, a platinum about a yard in length was formed into a spiral, and each end red to an insulated copper wire. The junctions were covered wax, and the wires, carefully insulated with wax, passed through holes in a cork into a bottle containing dilute sulphuric acid. ugh the same cork there passed a thermometer and two glass . The whole was placed in a large vessel of water. After ng saturated the wire with hydrogen, the acid was drawn off, ir drawn through for some time. The resistance was found to ase slightly on testing.

get rid of possible error from change of temperature, two wires qual length and section were used and balanced against each (see fig. 4).

FIG. 4.




ese were placed in water, and a current passed from the one to her, allowed to remain in the acid a little to cool if necessary, fterwards removed, dried, and placed in an empty glass vessel *nded with a considerable* quantity of water. There they rested *he temperature became the* same as the water. When measured

the resistance of the wire containing the hydrogen was found to l increased about one-thousandth part. It is not necessary to try effect of hydrogen on palladium, as the resistance is known t increased considerably by the absorbed gas.

*Carbon.*—Two thin rods about 2 mm. diameter were electropl at the ends and soldered to insulated copper wires. After protec the plated portion with marine glue, the whole was fixed convenient frame, and placed in dilute sulphuric acid. As was in the case of the platinum wires, the rods were balanced against other in order to eliminate changes of temperature, &c.

When used as the poles of a battery the change of resistance considerable, but greater on the rod that had been the positive By using a platinum electrode, hydrogen or oxygen was prod at will upon the same rod, the other rod remaining unchanged then appeared that oxygen increased the resistance much more hydrogen, rising in some cases as high as nine times; that v oxygen was liberated twice or thrice in succession, the resist increased each time. This continued increase was probably du chemical changes produced by the active oxygen. Hydrogen gav increase of resistance, not continuing beyond a certain point, and becoming greater on repeated charging with the gas.

Generally also the effect of the hydrogen was temporary, disapp ing, wholly in some cases, partially in others, when short circuite The following series of observations afford an example of this :-

| | | |
|---|---|---|
| 1. When rod A was charged with oxygen its resistance was | 4·15 | ohms. |
| 2. When rod A was charged with hydrogen its resistance was | 4·1633 | " |
| 3. When rod A was charged with hydrogen its resistance was | 4·1633 | " |
| 4. When rod A was charged with oxygen its resistance was | 4·2833 | " |
| 5. When rod A was charged with oxygen its resistance was | 4·2966 | " |
| 6. When rod A was charged with hydrogen its resistance was | 4·3099 | " |
| 7. Allowed to rest short circuited | 4·2966 | " |
| 8. Again charged with hydrogen | 4·3066 | " |
| 9. Allowed to rest | 4·303 | " |

In the case of hydrogen, the increase was 0·0133 ohm in experiments, and 0·01 in the other, while it recovered completely observation No. 6 and partially after No. 8.

*Superposition of Polarisations.*—Part of the change in the carb *evidently* produced by the mechanical action of the gases ev

he chemical action of the oxygen; both of these will, however, permanent changes. That only part of the action is to be d in this way is shown by the previous experiments. It is, , further demonstrated by using two carbon rods in decom-cidulated water; after passing the current for one minute, it for one-tenth of a second and immediately join up to a meter. A short but violent deflection appears for the latter gradually falling to zero and passing to the other side, where it for a considerable time, though with much decreased quantity. same thing was obtained with platinum electrodes. The contact must be very short, or the former polarisation irs. I have not yet succeeded in obtaining more than one , although I have no doubt that more may be got with very ectrodes.

*ance.*—Copper and iron absorb hydrogen and silver occludes but no change in their thermo-electric properties could be l. Carbonic oxide is absorbed by iron, and is said to produce hanges in its properties. In this case, however, only the ce was measured.

ce of iron wire, about 3 yards in length, was twisted into and placed in a porcelain tube; the ends projecting about s, were connected with one side of a bridge and balanced an equal spiral of the same wire. After exhausting the out 1 foot of the central portion was heated to a bright red-l then allowed to cool. Next day the resistance was measured, experiment repeated twice. On the third heating, carbonic as allowed to enter the porcelain tube, and readings of the ce taken on cooling as before. This was also repeated.

series was again repeated with new wires, and lastly, the wire sed to a bright red *in vacuo* and allowed to cool, the object remove the carbonic oxide gas in order that another measure-ight be taken after these repeated heatings. The resistance arly proving that part of the previous increase was due to the e of the gas. No measurement of resistance was taken on e day that the wires were heated, but at least 15 hours were to elapse.

series of observations give the numbers thus:—

ge of three measurements after heating *in vacuo*, 0·4 ohm.
" " " " in carbonic oxide, 0·41 "

e new wire—

ge of three measurements after heating *in vacuo*, 0·63 "
" " " " in carbonic oxide, 0·655 "

*heating in vacuo to* expel the gas, it fell to 0·642 "

These experiments appear to prove that absorbed gases increase the resistance of conductors, and that hydrogen renders metals more negative (thermo-electrically) whilst carbon becomes more positive.

I have introduced the experiment (fig. 1) which caused this work to be undertaken, although I do not think that it is entirely caused by the occlusion of gases, where the best results are obtained by electrolysis which produces them in a nascent or more energetic state.

*Thermo-electric and other Properties of Graphite and Carbon.*

In making the previous experiments, I had occasion to place the heated end of one carbon rod in contact with the cold end of another. The temperature of the hot end was varied from 30° C. to a red heat, whilst the cold end was kept at about 17° C.

Currents of electricity were of course produced. When the temperature of the hotter rod was raised but slightly, the current was from cold to hot through the point of contact, but when it was raised to a red heat the current passed from hot to cold; between these temperatures the direction of the current varied, appearing at first sight to obey no rule, and as nothing was known that would explain these results, I was led to examine the matter more carefully.

There were several difficulties to be overcome before any satisfactory results could be obtained.

Firstly, it was necessary to get two rods of such pure material, that they would not produce a current when placed in contact end to end and heated, or at any rate weak enough to be neglected in presence of that produced by the contact of the two rods at different temperatures.

I tried several specimens of gas-carbon, but as no two pieces were found to fulfil the condition before mentioned, they were useless. I was more fortunate with the rods prepared for arc lamps in electric lighting, readily finding two that answered my purpose.

A small portion of one of them gave on combustion less than one part of incombustible matter in 200 of carbon. They were heated repeatedly to a red heat and allowed to cool slowly. The ends were filed flat to prevent difference of shape producing any current.

When placed in contact end to end and heated, one rod was slightly positive to the other, but not sufficiently to prevent the experiments from succeeding.

Secondly, the manner of making contact caused the currents to vary much in strength, and the surface of the heated rod required filing at intervals, in order to preserve a clean flat face.

*It was found also* that the heat of the hot rod passed so quickly to *the cold one that* even after a very short contact the current fell, so *that the rods could* be placed together once only and for a very shor

time; after which they require to be brought back to their original temperature.

Lastly, to avoid any possible effect from the coal-gas, the end to be heated was inclosed in an iron tube lined with asbestos.

The temperatures were measured in various ways. In some experiments an ordinary thermometer was used for temperatures below 250° C.; thermo-electric couples of platinum and copper, silver and copper, were tried, but, although much more tedious, I found the method of platinum wire much less liable to error.

The wire was given to me by Mr. H. F. Callendar, M.A., and was from the same piece as that used by him in his experiments on "The Practical Measurement of Temperature" (see 'Phil. Trans.,' vol. 178 (1887), p. 161).

The following equations for this wire were used in determining the temperature, and are those obtained by Mr. Callendar in his experiments:—

$$\frac{R^t}{R^0} = 1 + 0\cdot00346\,Pt^\circ.$$

$$t^\circ - Pt^\circ = 1\cdot57\left\{\left(\frac{t}{100}\right)^2 - \frac{t}{100}\right\}.$$

$R^t$ = resistance of the platinum wire at $t^\circ$ C.
$R^0$ = " " " 0° C.

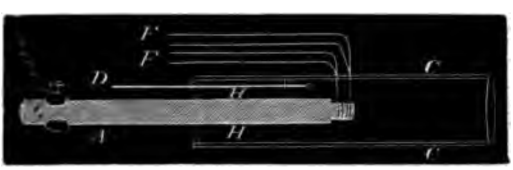
The wire was arranged as in fig. 5, by which means the resistance of

FIG. 5.

EF alone could be obtained by observing those of AC, BD, CD, and AB; also AB and CD were known if required, which indeed was the case of one of the later experiments.

In some cases the insulation was thin tubes of hard glass, in others the wire was wrapped up in thin sheet asbestos. The arrangement is shown in figs. 6 and 6*a*, where A and B are the carbon rods, C an

FIG. 6.



iron tube lined with sheet asbestos, H, H packing of asbestos, D a thermometer for moderate temperature and to test the calculations

FIG. 6a.


from the platinum wire, F, F platinum wire insulated; W a vessel of water containing a brass tube E, closed at one end, in which the carbon rod B is placed after each contact.

During the first series of experiments the temperature of W, and hence of B, was 16° C., that of A was changed in each contact, rising to 480° C. and higher. At about 480° the deflection changed; decreasing on approaching that temperature, and changing sign above it. I am sorry to say that the difficulty of obtaining the same perfection in each contact was so great that the deflections, although increasing above 480°, were not sufficiently consistent to allow a curve to be drawn.

Therefore, assuming that the neutral point was midway between that of the two rods when no current was produced (*i.e.*, 16° C. and 480° C.) we get 248° C. for the temperature of that point.

B being kept in the second series at 50°, in the third at 100°, and in the fourth at 200°, and the same assumption made in the calculation as before, 255° C. was given as the neutral point. If we now rule a line such that any two points being taken in it, the current shall be equal to the vertical distance between them, and shall flow from the higher point to the lower, it will have its lowest point at from 248° to 255°, rising to 0° and 480° and above (see fig. 7). This assumes that the two lines are equally inclined, and from the experiment with a platinum-carbon couple we judge them to be so, and their turning point to be 250° C.

From the preceding experiments I was led to expect that the line of carbon in a thermo-electric diagram, in which the area of the space between the lines is proportional to the electromotive force, would show a bend of some kind, and as no researches were known showing such a bend, it appeared desirable to test it carefully.

There is a paper by E. Becquerel in which he gives an account of a

number of experiments with various bodies, among which is gas-carbon. The hot junction was 100° C., at which temperature the deflection produced by a couple (carbon and copper) was negative, the same as copper-platinum, but a little larger. He does not appear to have worked at higher temperatures ('Annales de Chimie,' vol. 8, 1866, p. 415).

Knott and MacGregor also worked with gas-carbon, and in 1879 published a paper in the 'Transactions of the Royal Society of Edinburgh,' vol. 28, in which a line for carbon is given. The material was in the form of a cylinder 15 cm. long, 1·5 cm. thick. A strong heated wrought-iron tube, 4 inches long, 2 inches diameter, and 1-inch bore, closed at one end, was suspended over the junction and allowed to cool gradually.

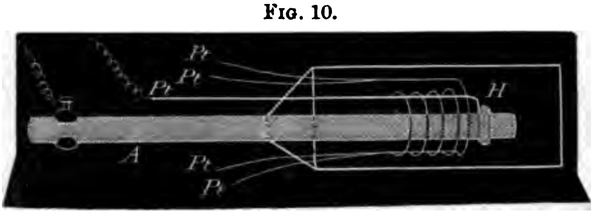
From 230° downwards the line is parallel to that of platinum. Above 230° it appears somewhat uncertain; they speak of it thus:—"For a small range of temperature (to 230° C.) it is possible to express the deflection in terms of the first and second powers of the temperature, the following formula holding good: $\delta = -8{\cdot}29 + 0{\cdot}604\,t + 0{\cdot}000385\,t^2$; above 230° C. it does not, perhaps because of chemical changes, produced by heat. Carbon appears to be an exception to the general law." "The above formula and the graphic treatment enable us at a higher temperature to determine its position" (see fig. 8). The position and slope of the lines are opposite to those now used.

Such a result did not appear to agree with the experiments already described, and as I had found gas-carbon a very unsuitable body for use where two pieces were required having anything like the same thermo-electric power, it appeared probable that good results might be got with the other rods; and as carbon and platinum form for 230° parallel lines I decided to use a couple consisting of these two bodies.

Nine series of observations were taken, using three different methods, of which it will be sufficient to describe the last.

Near one end of a carbon rod a hole, about 5 mm. in diameter, was drilled, and into this the end of a platinum wire was inserted and fixed by being wedged with a piece of rod carbon. The whole was thoroughly covered with Indian ink, which, when dry, was again

Fig. 10.



covered with clay. The carbon rod was insulated from the platinum wires, and they from each other by thin sheet asbestos and mica, by which means it was insulated from the vessel in which it was placed, and luted with clay to prevent access of air (fig. 10). The numbers obtained in three series are—

| Expt. 1. *t.* | E. in micro-volts. | Expt. 2. *t.* | E. | Expt. 3. *t.* | E. |
|---|---|---|---|---|---|
| 50 | 270 | 220 | 1800 | 210 | 1620 |
| 70 | 450 | 344 | 3240 | 312 | 3024 |
| 88 | 540 | 499 | 5760 | 471 | 5292 |
| 107 | 720 | 620 | 7560 | 635 | 8154 |
| 130 | 900 | 700 | 9900 | 722 | 9990 |
| 160 | 1260 | | | | |
| 180 | 1440 | | | | |
| 210 | 1620 | | | | |

The colder junction was at 17° C.

The resistance of the Pt-C couple was found to vary, increasing to 600°, after which it decreased. This result being caused by the increased resistance of the platinum being partly neutralised by the diminution of the resistance of the carbon, to which must be added the improved contact obtained by the expansion of the platinum in the carbon, which is greater than the expansion of the carbon, thence the pressure increases and the contact improves.

The numbers were at 220° C. 0·88 ohm, 340° to 500° C. 0·92 ohm, 620° C. 1·03, 700° C. 1·00.

These experiments agree perfectly with the diagram given by Knott and Macgregor (fig. 8) as far as they carried it experimentally. When, however, they commence deducing results for higher temperatures, our experiments are not in accord; there being no indication of the carbon line crossing the platinum line, but only a very slight indication in one of the series of an approach above 230°.

Assuming the platinum line for our wire to be the same as that given in Tait's diagram (Fleeming Jenkin, p. 178) we get a diagram for carbon (fig. 8A), in which the line is fairly parallel to 250° C., after which it gradually increases its distance.

*Other Changes in the Properties of the Body at the same Temperature.*

This change in the thermo-electric power of carbon is accompanied by other changes. The resistance, the expansion, and the specific heat all appear to undergo a corresponding alteration.

*Resistance.*—Accurate measurements of the resistance of carbon at high temperatures are very difficult to obtain, owing to the changes that take place in the connexions. It is desirable, if possible,

that the whole rod should be exposed to the same temperature. If the rods are thick the changes in the contacts, even at ordinary temperatures, become great in proportion to the resistance of the rods; and if thin there is great danger of them being changed by the heat.

We found the method of electroplating with copper very good up to 500° or 600°, after which it completely broke down, and we were not able to get any other method to stand. Thus the experiments were stopped there, although we expected other changes at 800° to 1000°, from the numbers obtained for the specific heat by Weber.

The first method tried was that used by H. Muraska ('Annalen der Physik und Chemie,' vol. 13, 1881, p. 310), in which a hole is drilled in each end of the carbon rod, and after electroplating with copper, a copper rod is pushed in tight and brazed in. The objections to this method were: 1st, requires a thick rod; 2nd, better contact formed as the temperature rises, tending to produce error in the same direction as the results of the experiments.

Second. Forming a contact that would be liquid at all temperatures above 100°. This was done by drilling vertical holes near the ends of the rods, and filling them with fusible metal. Required thick rods, gave way.

Third. Used thin rods so that the change in contact resistance might not bear so large a proportion to that of the rod itself. Glass vessels shaped as in fig. 11 were prepared, and the rod packed at A

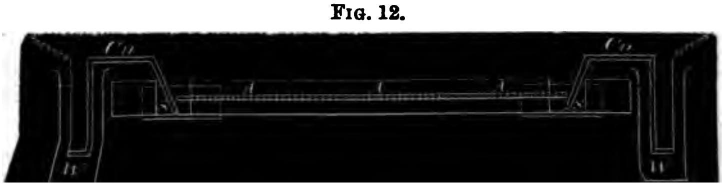
FIG. 11.

and B with asbestos. Fusible metal or solder was melted into the glasses, and the rod protected by a glass tube B.

Fourth. An attempt was made to form contacts by inserting the thin rod into cavities drilled into thick rods of carbon, and joining by Indian ink, sugar and graphite, &c.

Lastly, the rod was incased in thin sheet asbestos, well coated with wet clay between each layer. The ends were electroplated with copper and tinned. They projected beyond the asbestos covering

FIG. 12.



inch. The glass tubes in the previous method were imitated ıtos, and into the spaces S, S solder was melted, and thick wires inserted, the other ends of which were kept cool by When taking observations at high temperatures it is better r this with a glass tube at the portion AA. Out of a large f readings we give four.

*ıite Rods.*—These rods were supplied by Hogarth and Hayes rick as pure natural Cumberland graphite.

th, 7¾ inches; diameter, 0·155 inch.

Experiment 1.

| Time of observation. | Temperature. | R. in ohms. |
|---|---|---|
| 10 | 21° | 42·3 |
| 12.15 | 600 | 23·8 |
| 12.25 | 412 | 29·7 |
| 12.50 | 278 | 33·72 |
| 3.35 | 21 | 42·3 |

Experiment 2.

| | | |
|---|---|---|
| 11 A.M. | 22° | 30·4 |
| 12.50 | 155 | 27·0 |
| 2.55 | 202 | 26·2 |
| 4.30 | 278 | 25·5 |
| 5.54 | 390 | 23·2 |
| ıy, 10.45 | 22 | 31·0 |

*m Rods.*—Carbon rods supplied by Woodhouse and Rawson, a Street, London. Very hard and good, 12 inches long; ır, 0·22 inch.

Experiment 3.

| Time of observation. | Temperature. | R. in ohms. |
|---|---|---|
| 3.15 P.M. | 347° | 4·75 |
| 5 | 309 | 4·75 |
| 6.40 | 298 | 4·81 |
| 7.35 | 257 | 4·85 |
| 8 | 226 | 4·88 |
| day 10 A.M. | 23 | 5·2 |

Experiment 4.

| | | |
|---|---|---|
| 12.15 | 325 | 4·74 |
| 2.30 | 273 | 4·83 |
| 4.10 | 221 | 4·90 |
| 5 | 202 | 4·93 |
| day 11 A.M. | 22 | 5·21 |

Changes per 1° C. per 1 ohm—

Expt. 1 gives—

| Temp. | Change |
|---|---|
| 21 | |
| | 0·0009 |
| 180 | |
| | 0·00068 |
| 278 | |
| | 0·00070 |
| 412 | |
| | 0·00076 |
| 600 | |

Expt. 2 gives—

| Temp. | Change |
|---|---|
| 22 | |
| | 0·0008 |
| 155 | |
| | 0·000678 |
| 202 | |
| | 0·00038 |
| 278 | |
| | 0·00052 |
| 391 | |

Expt. 3 gives—

| Temp. | Change |
|---|---|
| 23 | |
| | 0·00031 |
| 226 | |
| | 0·00025 |
| 257 | |
| | 0·000195 |
| 298 | |
| | 0·00032 |
| 347 | |

Expt. 4 gives

| Temp. | Change |
|---|---|
| 22 | |
| | 0·00031 |
| 221 | |
| | 0·00026 |
| 273 | |
| | 0·00030 |
| 325 | |

All showing a decrease (in the temperature coefficient) to about 250°, and then an increase.

This method cannot lay claim to absolute accuracy, as there is in some cases an increase of resistance by the change in the contact of copper with carbon, which appears when the rod cools as in Experiment 2. This, however, takes place at the higher temperatures, and tends to decrease the numbers obtained at those temperatures, and a correction, if one could be applied, would only increase the results obtained in the previous experiments.

*Coefficient of Expansion.*

*Method.*—As we wished to raise the rod to 500° or 600° C., it was impossible to expose the whole rod to that temperature, and at the same time to read the changes of position of a mark or point at the end of it with a microscope; nor did it appear probable that contact could be made by rods of other materials.

It was decided, therefore, to heat the central portion of a rod, keeping the end portions cold. We had thus one hot portion, two colder, and two others at a constant temperature. A rod, about 36 inches in length and ½ inch in diameter, was used. One end was electroplated and then soldered into a cavity in a brass rod which was firmly clamped to a vertical iron one fixed to a stone table. Into a small hole in the other end a fine needle was fixed whose change of position was read by a microscope.

The central portion of the carbon was covered with a thin coating of clay, then with paper to consume the oxygen, outside that a glass tube packed with asbestos inside of a porcelain tube.

Ten inches of the centre of this was heated in a gas furnace. The temperature was taken with a platinum thermometer (fig. 5), EF giving the temperature of the hottest part, AB and CD those of the portions between the hottest and the constantly cold portion.

EF was 10 inches, AB and CD 7 inches each, total 24 inches. Outside the rod was kept cool with water.

In calculating the portion of the expansion due to the parts AB and CD the numbers obtained in Experiment 4 are used. The expansion is assumed to be regular up to 143°, the number obtained from this is used for the cooler portions AB and CD up to 98°; above that, the number found in the same experiment for the expansion between 143° and 263° is used.

One example will show what is meant. In Experiment 4, observation 1, we have—

$$\left.\begin{array}{ll} \text{AB} \ldots\ldots & 54^\circ \\ \text{CD} \ldots\ldots & 29 \\ \text{EF} \ldots\ldots & 143 \\ \text{Cold portion} & 15 \end{array}\right\} \quad \begin{array}{rcrcr} 54-15 &=& 39\times 7 &=& 273 \\ 29-15 &=& 14\times 7 &=& 98 \\ 143-15 &=& 128\times 10 &=& 1280 \\ \hline & & & & 1651 \end{array}$$

$$\frac{0\cdot 0075}{1651} = 0\cdot 0000045.$$

Table showing the Temperature of each Portion of the Rod at each Observation, the total Change in Length, and the Coefficient of Expansion.

| AB. | EF. | CD. | Cold part. | Total expansion. | Coefficient of expansion. |
|---|---|---|---|---|---|
| | | | | in. | |
| Expt. 1. 180° | 614° | 263° | 13° | 0·057083 | 0·00000666 between 13° and 614° |
| „ 2. 208 | 645 | 263 | 14 | 0·059375 | 0·0000066 „ 14 „ 645 |
| „ 3. { 101 | 300 | 89 | 15 | 0·021041 | 0·0000056 „ 15 „ 300 |
| „ 3. { 208 | 645 | 167 | 15 | 0·058541 | 0·000008 „ 300 „ 645 |
| Expt. 4. { 54 | 143 | 29 | 15 | 0·0075 | 0·0000045 „ 15 „ 143 |
| Expt. 4. { 86 | 263 | 44 | 15 | 0·0183′ | 0·0000077 „ 143 „ 263 |
| Expt. 4. { 98 | 282 | 19 | 15 | 0·0216′ | 0·0000140 „ 263 „ 282 |
| Expt. 4. { 194 | 602 | 167 | 15 | 0·0583′ | 0·000009 „ 282 „ 602 |

Nos. 1 and 2 give the average of the whole of No. 4, and part 1 of No. 3 is not far removed from the average of parts 1 and 2 of 4, while part 2 of No. 3 is lower than the number obtained in No. 4.

*Specific Heat.*—H. F. Weber gives the following numbers as the specific heat of carbon at various temperatures; unfortunately for our purpose, no observations are recorded between 250° and 640°.

| | Temperature. | Specific heat. | Rate of change per 1° C. |
|---|---|---|---|
| | −50·3° | 0·1138 | 0·00075 |
| Graphite | −10·7 | 0·1437 | 0·00076 |
| | 61·3 | 0·1990 | 0·00071 |
| | 138·5 | 0·2542 | 0·00067 |
| | 201·6 | 0·2966 | 0·00063 |
| | 249·3 | 0·3250 | 0·00030 |
| | 641·9 | 0·4454 | 0·000045 |
| | 822 | 0·4539 | 0·000083 |
| | 977 | 0·467 | |

The curve, fig. 9, is plotted from these numbers and shows a fairl regular increase in the specific heat with the temperature up to 25( where the line bends; another bend occurs at 650°.

Fig 9. Showing the Increase of Specific Heat of Graphite.

Other changes were looked for at the higher temperature, but tl contacts gave way, and no definite results were obtained. In co clusion I wish to acknowledge my obligations to Professor J. Thomson, F.R.S. and to R. T. Glazebrook, F.R.S., for much inform tion and advice during the whole course of the work.

Summary of Results.

| | Below 250° C. | Above 250° C. |
|---|---|---|
| A. Effect of contact of hot and cold carbon. | Current from cold to hot. | Current from hot to cold. |
| B. Thermo-electric line | Rises. | Falls. |
| C. Rate of decrease of resistance per degree per ohm. | Diminishes. | Increases. |
| D. The rate of increase of the coefficient of expansion. | Increases. | Decreases. |
| *E. Rate of increase of the specific heat.* | Fairly regular. | Falls to half. |

II. "Colour Photometry. Part II. The Measurement of Reflected Colours." By Capt. W. de W. ABNEY, R.E., F.R.S., and Major-General FESTING, R.E., F.R.S. Received May 3, 1888.

(Abstract.)

In a previous paper we showed how the luminosity of different pectrum colours might be measured, and in the present paper we ive a method of measuring the light of the spectrum reflected from oloured bodies such as pigments in terms of the light of the pectrum reflected from a white surface. To effect this the first amed of us devised a modification of our previous apparatus. Tearly in contact with the collimating lens was placed a double image rism of Iceland spar, by which means two spectra were thrown on he focussing screen of the camera (which was arranged as described n the Bakerian Lecture for 1886), each formed of the light which nters the slit. The light was thus identical in both spectra. The wo spectra were separated by about $\frac{1}{8}$ of an inch when the adjustments were complete. A slit cut in a card was passed through his spectrum to isolate any particular portion which might be equired. The rays coming from the uppermost spectrum were eflected by means of a small right-angled prism in a direction nearly t right angles to the original direction on to another right-angled rism. Both prisms were attached to the card. From this last prism he rays fell on a lens and formed on a white screen an image of the ace of the spectroscope prism in monochromatic light. The ray of the ame wave-length as that reflected from the upper spectrum passed hrough the lower half of the slit, and falling on another lens formed another image of the face of the prism, superposed over he first image. A rod placed in front of the screen thus cast two shadows, one illuminated by monochromatic rays from the top spectrum, and the other by those from the bottom spectrum. The illumination of the two shadows was equalised by means of rotating sectors which could be closed and opened at pleasure during the time of rotation. The angle to which the sector required to be opened to establish equality of illumination of the two shadows gave the ratio of the brightness of the two spectra. When proper adjustment had been made the relative brightness was the same throughout the entire spectrum.

To measure the intensity of any ray reflected from a pigment, a paper was coated with it and placed adjacent to a white surface, and it was so arranged that one shadow of the rod fell on the coloured urface and the other on the white surface. The illuminations were

then equalised by the sectors and the relative intensities of the two reflected rays calculated. This was repeated throughout the spectrum. Vermilion, emerald-green, and French ultramarine were first measured by the above method and then sectors of these colours prepared, which when rotated gave a grey matching a grey obtained by rotation of black and white. The luminosity curves of these three colours were then calculated and reduced proportionally to the angle that each sector occupied in the disk. The luminosity curve of the white was then reduced in a similar manner, and it was found that the sum of the luminosities of the three colours almost exactly equalled that of the white. The same measurements were gone through with pale-yellow chrome and a French blue, which formed a grey on rotation, with like results. It was further found that *the sum of the intensities* of vermilion, blue, and green varied at different parts of the spectrum, and the line joining them was not parallel to the straight line which represented white for all colours of the spectrum and which itself was parallel to the base. Since a straight line parallel to the base indicated degraded white, it followed that if the intensity of the rays of the spectrum were reduced proportionally to the height of the ordinates above a line tangential to the curved line (which represented the sum of the intensities of the three colours at the different parts of the spectrum) and were recombined, a grey should result. A method was devised of trying this, and the experiment proved that such was the case. The same plan enabled the colour of any pigment to be reproduced from the spectrum on the screen. The combination of colours to form a grey on rotation by a colour-blind person was also tried, and after the curve of luminosity of the colours had been calculated and reduced according to the amount required in the disk, it was found that the sum of the areas of the curves was approximately equal to the white necessary to be added to a black disk to form a grey of equal intensity as perceived by him. The spectrum intensity of gaslight in comparison with the electric light was also measured, and the amount of the different colours necessary to form a grey in this light was ascertained by experiment.

As before, it was found that the calculated luminosity of the colours was equal to the white which combined with black formed a grey of equal luminosity.

The question of the coloured light reflected from different metals was next considered, and the method of measuring it devised, as was also the method of measuring absorption spectra. The luminosity curves obtained by the old method were compared with those obtained by the present method, and so close an agreement between them was found to exist, as to give a further confirmation that our former plan was accurate. A number of pigments that can be used for

forming greys by rotation were measured, and the results tabulated in percentages of the spectrum of white light and on a wave-length scale.

## III. "The Conditions of the Evolution of Gases from Homogeneous Liquids." By V. H. VELEY, M.A., University College, Oxford. Communicated by A. VERNON HARCOURT, M.A., F.R.S. Received May 5, 1888.

(Abstract.)

This paper is conveniently divided into three parts. In part (i) an account is given of the effect of finely divided particles on the rate of evolution of gases resulting from chemical changes; in part (ii) the phenomenon of initial acceleration, as also the effect of variation of pressure on the evolution of gases, is discussed; in part (iii) the case of the decomposition of formic acid into carbonic oxide and water is investigated under constant conditions, other than those of the mass of reacting substances and of temperature.

*Part I.*—It is found that the addition of finely divided chemically inert particles increases the rate of evolution of gases from liquids in which they are being formed. The effect of these particles on the following chemical changes is investigated: (i) the decomposition of formic acid yielding carbonic oxide; (ii) the decomposition of ammonium nitrite in aqueous solution yielding nitrogen; (iii) the reduction of nitric acid into nitric oxide by means of ferrous sulphate; (iv) the decomposition of ammonium nitrate in a state of fusion producing nitrous oxide; and (v) the decomposition of potassium chlorate in a state of fusion producing oxygen. The finely divided substances used are pumice, silica, graphite, precipitated barium sulphate and glass-dust.

*Part II.*—It is observed that, conditions of temperature remaining the same, the rate of evolution of a gas from a liquid is at first slow, then gradually increases until it reaches a maximum and for some time constant rate. From this point the rate decreases proportionally to the diminution of mass. This is observed in the cases of the decomposition of formic acid, potassium ferrocyanide, and of oxalic acid by concentrated sulphuric acid, and in that of ammonium nitrate. It has previously been observed in the case of the decomposition of ammonium nitrite in aqueous solution. The same phenomenon repeats itself when the temperature is temporarily lowered and then raised to its former point, and also to a more marked degree when, temperature remaining the same, the superincumbent pressure is suddenly increased.

*The reduction of pressure* from one to a fraction of an atmosphere

produces no *permanent* effect on the rate of evolution of a gas from a liquid, a decrease of pressure, however, produces *temporarily* an increase in the rate, and an increase of pressure conversely produces *temporarily* a decrease in the rate.

*Part III.*—The case of the decomposition of formic acid into carbonic oxide and water by diluted sulphuric acid is studied with the aid of an apparatus by means of which the temperature is kept constant within one-twentieth of a degree. It is shown that the rate of evolution of carbonic oxide is expressible by the following equation:—

$$\log(\tau + t) + \log r = \log c,$$

in which $\tau$ is the time from the commencement of the observations; $t$ is the interval of time from the moment of commencement, and that at which, conditions remaining the same, the interval of time required for unit change would have been *nil*; $r$ is the mass at the end of each observation, and $c$ is a constant. The results calculated by this hypothesis agree with those observed, whether the interval of time required for unit change is 30 or 960 minutes. The curve expressing the rate of chemical change in terms of mass is thus hyperbolic and illustrative of the law

$$\frac{dr}{d\tau} = -\frac{r^2}{c},$$

which expresses the rate at which equivalent masses act upon another; $1/c$ in each experiment is the amount of each unit mass which reacts with the other per unit of time, when an unit mass of each substance is present. Since then equivalent masses take part in the change, it is reasonable to suppose that at first an anhydride of formic acid is produced thus:—

$$\left.\begin{matrix}HCO\\H\end{matrix}\right\}O + \left.\begin{matrix}HCO\\H\end{matrix}\right\}O = \left.\begin{matrix}HCO\\HCO\end{matrix}\right\}O + H_2O.$$

The anhydride is unstable, and is subsequently decomposed into carbonic oxide and water,

$$\left.\begin{matrix}HCO\\HCO\end{matrix}\right\}O = 2CO + OH_2.$$

The change may thus be compared to the production of ethyl formate from formic acid and alcohol,

$$\left.\begin{matrix}HCO\\H\end{matrix}\right\}O + \left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O = \left.\begin{matrix}HCO\\C_2H_5\end{matrix}\right\}O + H_2O.$$

with which it shows several points of analogy.

*In the original* paper the methods of observation and the apparatus *used are described* in full, and the results obtained are set forth in a *series of tables.*

IV. "Investigations on the Spectrum of Magnesium. No. II." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received May 16, 1888.

Since our last communication on this subject, we have made many additional observations on the spectrum of magnesium under various circumstances, and have arrived at some new results. Speaking generally, we find that differences of temperature, such as we get in the flame of burning magnesium, in the arc, and in the spark, produce less differences in the spectrum than we had before attributed to them. For instance, the lines which previously we had observed only in the spark discharge, we have since found to be developed in the arc also, provided the discharge occur between electrodes of magnesium.* In making these experiments we used thick electrodes of magnesium, and brought them together inside a glass globe about 6 inches in diameter, fitted with a plate of quartz in front and filled from time to time with various gases. The arc was an instantaneous flash which could not be repeated more than twice without rendering the sides of the vessel opaque with a complete coating of magnesium. It was therefore analogous to an explosion of magnesium vapour. The strong blue line λ4481, two pairs about λ3895, 3893, and λ3855, 3848, the strong pair about λ2935, 2927, and the two weaker lines of the quadruple group, namely, λ2789·9 and 2797, all come out in the arc given by a Siemens' dynamo between magnesium electrodes in air, in nitrogen, and in hydrogen. We have observed most of them also when the arc is taken in carbonic acid, in ammonia, in steam, in hydrochloric acid, in chlorine, and in oxygen. The relative intensities of these lines, as compared with one another and with the other lines of the spectrum, vary considerably under different circumstances, of which temperature is doubtless one of the most important; but none of the spark lines seem to be absent from the arc, and even the blue line λ4481, so characteristic of the spark, which we never found in the electric arc taken between carbon poles in a crucible of magnesia even on addition of magnesium, is sometimes quite as strongly shown in the arc between magnesium electrodes. There are still several lines of the arc which we have never observed in the spark, such as the series of triplets of wavelength less than 2770, but their presence may be dependent more on the large quantity of incandescent matter in the arc than upon its relative temperature. The observations, however, render doubtful

* *Compare the appearance of the lines* of hydrogen in the arc discharge, 'Roy. *Soc. Proc.,' vol. 30, p. 157; and vol. 35, p. 75.*

the correctness of the received opinion that the temperature of the spark discharge is much higher than that of the arc. The greater mass of the incandescent matter in the arc may be expected to give a greater number of lines, because the gradations of temperature will be less steep than in a smaller mass, and we shall have from the outer part of the mass the light which is emitted at comparatively low temperatures, while from the inner part we shall get those rays which are only produced by the highest temperatures. Moreover, compounds which may be dissociated in the interior of the mass may be re-formed in the outer part, and produce their characteristic emission or, in some cases, absorption spectra. Heat, however, is not the only form of energy which may give rise to vibrations, and it is probable that the energy of the electric discharge, as well as that due to chemical change, may directly impart to the matter affected vibrations which are more intense than the temperature alone would produce.

*The Bands of the Oxide.*

The set of seven bands in the green, beginning at about λ5006·4 and fading towards the violet side of the spectrum, which we have before attributed to the oxide of magnesium, have been subjected to further observation, and we have no reason to doubt the correctness of our former conclusion that they are due either to magnesia or to the chemical action of oxidation. On repeating our experiments with the spark of an induction coil between magnesium electrodes in different gases at atmospheric pressure, we could see no trace of these bands in hydrogen, nitrogen, or ammonia, whether a Leyden jar was used or not. Nor could we see them at all in carbonic oxide but in this case the brightness of the lines due to the gas might prevent the bands being seen if they were only feebly developed. On the other hand, the bands come out brilliantly when the gas is oxygen or carbonic acid, both with and without the use of a Leyden jar. In air and in steam they are less brilliant, but may be well seen when no jar is used. When a jar is used they are less conspicuous, because in air the lines of nitrogen come out strongly in the same region, and in steam the F line of hydrogen becomes both very bright and much expanded.* It seems, therefore, that it is not the character of the electric discharge, but the nature of the gas which determines the appearance of the bands; and the absence of

* Neither the arc of a Siemens' dynamo, nor that of a De Meritens' magneto electric machine, when taken in a crucible of magnesia, shows these bands, even if metallic magnesium be dropped into it. A stream of hydrogen led into the crucible with a view to cool it does not elicit them. When the arc is taken in the open air, and metallic magnesium dropped through it, the bands appear momentarily, but that is probably the result of the burning of the magnesium vapour outside the arc.—*May 23.*

the bands in the absence of oxygen, and their increased brilliance in that gas, leave little room for doubt that they are due to the oxide, or to the process of oxidation. It may be assumed that at a sufficiently high temperature magnesia will be decomposed, but magnesia is a very stable compound, a great amount of heat is developed in its formation, and it probably requires a temperature far above that of burning magnesium for its complete dissociation. This is consistent with the appearance of the bands in the spectrum of the flame of the burning metal, as well as in the condensed spark when the other conditions are favourable for the formation of the oxide, or for its stability when formed. In our earlier observations, we obtained in the *visible* region nothing but a continuous spectrum from magnesia heated with the oxyhydrogen blowpipe; neither the *b* group, nor $\lambda 4570$, nor the triplet near L appeared, but at the same time $\lambda 2852$ was not only strong, but was strongly reversed. We now find that this result, so far as it was negative, was a consequence of using too large a mass of magnesia to be adequately heated by the flame. If the piece of magnesia is very small, such as a fragment of the ash of burnt magnesium ribbon, most of the spectrum of burning magnesium is developed in the flame for a short distance from the piece of magnesia. It was not very easy to make these experiments successfully. About 3 inches of magnesium ribbon were burnt in air, and the ash carefully heated in the upper part of the oxyhydrogen flame to render it dense. The thread of magnesia so obtained was held horizontally with its end projecting into the oxyhydrogen flame so as to approach the boundary of the inner cone, and if the current of gas were not too strong all that was further necessary was to move up the thread horizontally as the end was worn away. When the magnesia was placed as described, the whole upper part of the flame was of a fine azure-blue colour. Under these circumstances, the flame shows the *b* group and the magnesium-hydrogen series close to it, the bands in the green, the triplet near L, the triplet near M of the flame of burning magnesium, with the group of bands in that region, and the line $\lambda 2852$. It is remarkable that the proportions in which the oxygen and hydrogen are mixed affect the relative intensities of different parts of the spectrum. In general, both the metallic lines of the *b* group and the bands of the oxide are easily seen; but if the oxygen be in excess the bands of the oxide come out with increased brightness, while the *b* group fades or sometimes becomes invisible. On the other hand, if the hydrogen be in excess the bands fade, and the *b* group shows increased brilliance. There can hardly be much difference in the temperature of the flame according as one gas or the other is in excess, but the excess of oxygen is favourable to the formation and stability of the oxide, while excess of hydrogen facilitates the reduction of magnesium and

its maintenance in the metallic state. As regards temperature, it should be observed that while substances merely heated by the flame, and not undergoing chemical change, are not likely to rise to a temperature above the average temperature of the flame, it will be otherwise with the materials of the flame itself and other substances in it which are undergoing chemical change, and have at the instant of such change the kinetic energy due to the change.

In a recent communication to the Society, "Researches on the Spectra of Meteorites," Mr. Lockyer has directly connected the appearance in nebulæ of these bands, namely, "the magnesium fluting at 500" with the temperature of the Bunsen burner ('Roy. Soc. Proc.,' vol. 43, p. 133). That the bands are persistent through a large range of temperature there is no doubt, but we cannot help thinking that Mr. Lockyer is mistaken in supposing them to be produced at the temperature of a Bunsen burner. It does not follow because the bands are seen when magnesium is burnt in a Bunsen burner that the molecules which emit them are at the temperature of the flame. In the combustion of the magnesium the formation of each molecule o magnesia is attended with a development of kinetic energy which, i it all took the form of heat and were all concentrated in the molecule must raise its temperature to very nearly the point at which magnesi is completely dissociated. The persistence of the molecule of magnesi when formed will depend upon the dissipation of some of this energy and one of the forms in which this dissipation occurs is the ver radiation which produces the bands. The character of the vibratio depends on the motions of the molecules, which in the case in questio are not derived from the heat of the flame, but from the stored energ of the separated elements, which becomes kinetic when they combine The temperature of complete dissociation of magnesia is very fa higher than any temperature which can reasonably be assigned to th Bunsen burner.

Nor do the observations we have made on magnesia in the oxy hydrogen flame appear to us to be inconsistent with the conclusio that the spectrum of the oxide is produced only at a high temperatur as we have a decomposition of magnesia by the hydrogen at th highest temperature of the blowpipe flame, and when hydrogen is i excess little but the metallic lines is visible, because the re-formatio of magnesia is, for the most part, the reversal of the former action, an occurs in the cooler part of the flame by the interchange of oxyge between steam and magnesium with scarcely any rise of temperatur On the other hand, when the oxygen is in excess the reduced magne sium carried up into the flame combines for the most part directl *with oxygen,* and individual molecules thereby acquire a motion o *far greater intensity* than they could derive from the average heat o *the flame.*

In fact, when chemical changes are occurring in a flame it cannot be taken for granted that the temperatures of the molecules are all alike, or that the vibrations which they assume are the result of heat alone. On the other hand, the temperature of the metal separated from magnesia by the oxyhydrogen flame cannot, we suppose, be at a temperature higher than that of the hottest part of the flame. We are therefore inclined to think that the metallic lines ($b$) are manifested at a lower temperature than the bands of the oxide; and the appearance of a line in the position of the first band without any trace of the second band (which is nearly as bright as the first), and without any trace of the $b$ group, is quite sufficient to create a suspicion of mistaken identity when Mr. Lockyer ascribes the sharp green line in the spectrum of nebulæ to this band of magnesia. This suspicion will be strengthened when it is noticed that the line in question is usually in the nebulæ associated with the F line of hydrogen, if it be borne in mind that the spark of magnesium in hydrogen does not give the bands, and that the oxyhydrogen flame hardly produces them from magnesia when the hydrogen is in excess.

In Mr. Lockyer's map of the spectrum of the nebula in Orion (*loc. cit.*, p. 134), he has represented three lines in the position of the edges of the first three of these bands. If these three lines were really seen in the nebula, there would be less room to doubt the identity of the spectra; but the authorities quoted for the map (*loc. cit.*, p. 142) mention only a single line in this position.

When the flame of burning magnesium is viewed with a high dispersion these bands are resolved into series of fine, closely set lines. Seven such series may be counted, beginning at the approximate wave-lengths 5006·4, 4995·6, 4985·4, 4973·6, 4961·6, 4948·6, 4934·4, respectively. When a condensed spark is taken between magnesium electrodes in oxygen mixed with a little air, the pair of strong nitrogen lines may be seen simultaneously with the bands, and lying within the first band, the bright edge of the band being somewhat less refrangible than the less refrangible of the two nitrogen lines.

When the bands are produced by the spark discharge between magnesium electrodes in oxygen or other gas, we have not been able to resolve them into lines, but the whole amount of light from the spark is small compared with that from the flame, and besides it is possible that the several lines forming the shading may be expanded in the spark, and thus obliterate the darker spaces between them.

### *Triplet near M and adjacent Bands.*

Our former account of the spectrum of the flame of burning magnesium included a description of a triplet near the solar line M, and a series of bands extending from it beyond the well-known triplet near

L. As we had not observed these features in the spectrum of the spark or arc, and could not trace their connexion with any compound, we concluded that they were produced by magnesium only at the comparatively low temperature of the flame. We have since found that they are not produced by the metal at that temperature only, but are exhibited as strongly, or even more strongly, in the arc between electrodes of magnesium. In the latter case they appear concurrently with the line at 4481 and other lines which seem to belong to high temperatures. We must therefore regard them as not only produced at the temperature of flames, but as persistent at temperatures very much higher.

The different circumstances under which we have observed this triplet are as follows:—

In the oxyhydrogen flame when a very small piece of magnesia is held in it. In this case the outer two lines of the triplet are much stronger than the middle line (λ3724 about), which in some of our photographs does not show at all. It should be noticed that the least refrangible of the three lines (λ3730 about) is in general more diffuse and not quite so bright as the two more refrangible lines. Magnesia in the oxyhydrogen flame also gives rise to some bands close to and more refrangible than the triplet, and to another still more refrangible but less bright triplet, in which the lines are set at nearly equal distances from each other, with the approximate wave-lengths 3633·7, 3626·2, 3620·6. These additional bands and triplets are not really absent from the flame spectrum, for traces of them may be seen in some of our photographs of the magnesium flame, but they seem relatively brighter in the oxyhydrogen flame with magnesia, and the longer exposure of the photographic plate in the latter case helped to bring them out. They seem to come out more strongly under the conditions which make both the green bands of the oxide and the *b* group show well.

The triplet near M is also produced when magnesium oxychloride and when magnesium chloride is substituted for magnesia in the oxyhydrogen flame, and in the former case the more refrangible triplet is developed as well.

When carbonic oxide and oxygen are substituted for hydrogen and oxygen, both triplets are developed in the part of the flame near the magnesia, and in this flame the middle line of the triplet near M (λ3724 about) is as strong as it is in the flame of burning magnesium.

The proper adjustment of the thread of magnesia in this flame is a much more delicate matter than in the oxyhydrogen flame. In fact, we made many experiments which were failures before we succeeded in getting satisfactory results; and latterly, in order to be certain of success, we had to fill a gas-holder with a mixture of carbonic oxide

and half its volume of oxygen and burn the gases as they issued from the holder.

We have not noticed the more refrangible triplet (λ3633·7 to 3620·6 about) under other circumstances, but the triplet near M is produced when magnesia is held in the flame of cyanogen burning in oxygen, in the flash of pyroxylin with which magnesium filings have been mixed, or which has been treated with an alcoholic solution of magnesium chloride.

It is not only very strongly developed, but shows strongly reversed on our photographic plates, in the spectrum of the arc from a Siemens' dynamo taken between electrodes of magnesium in oxygen; and most of the accompanying ultra-violet bands of the magnesium flame spectrum are at the same time reversed. It is less strongly, but distinctly, reversed in the spectrum of the same arc taken in air, in carbonic acid gas, and in sulphurous acid gas. It appears also if the arc is taken in ordinary nitrogen unless great precautions are taken to exclude all traces of oxygen or carbonic acid, when it completely disappears. It is developed also in the flash produced when a piece of magnesium ribbon is dissipated in air by the discharge through it of the current from 50 cells of a storage battery. Also in the spark in air at atmospheric pressure between magnesium electrodes connected with the secondary wire of an induction coil when the alternating current of a De Meritens' magneto-electric machine is passed through the primary.

In two cases, but only two, we have found this triplet, or what looks like one or both of the more refrangible of its lines, developed in vacuous tubes. In both tubes the gas was air. One had platinum electrodes and a strip of magnesia from burnt magnesium disposed along the tube; the other had fragments of the Dhurmsala meteorite attached to the platinum electrodes. The discharge was that of an induction coil worked in the usual way without a Leyden jar. In each case it is only in one photograph of the spectrum that the lines in question appear. In other photographs taken with the same tubes they do not show.

On the other hand, this triplet does not make its appearance in the arc from a dynamo between magnesium electrodes in hydrogen, coal gas, cyanogen,* chlorine, hydrochloric acid, or ammonia; nor in the

* In taking the arc in this way in cyanogen our photographs show the whole of the five bands of cyanogen between K and L well reversed. We have before noticed ('Roy. Soc. Proc.,' vol. 33, p. 4) the reversal of the more refrangible three of these bands against the bright background of the expanded lines of magnesium when some of that metal was dropped into the arc between carbon electrodes, but in taking the arc between magnesium electrodes in an atmosphere of cyanogen the bright wings of the expanded magnesium lines near L extend beyond the cyanogen bands, and the whole series of the latter are well reversed.—May 23.

arc from a De Meritens' machine in hydrogen or nitrogen. It does not show in the spark between magnesium electrodes of an induction coil used in the ordinary way, either with or without a Leyden jar, in hydrogen or in air at atmospheric pressure; nor in the glow discharge in vacuous tubes with magnesium electrodes when the residual gas is either air, oxygen, hydrogen, carbonic acid gas, or cyanogen. Nor does it appear, except in the one instance above mentioned, in the glow discharge in highly rarefied air in a tube containing either magnesia or a strip of metallic magnesium.

A review of all the circumstances under which the triplet near M and its associated bands appear, and of those under which they fail to appear, leads pretty conclusively to the inference that they are due not to merely heated magnesium but to the oxide, or to vibrations set up by the process of oxidation.

With reference to this triplet, Mr. Lockyer (*loc. cit.*, p. 122) has referred to us as his authority for the statement that at the temperature of a Bunsen burner as ordinarily employed the ultra-violet line visible is that at 373. We do not agree to this as a statement of observed fact, and we cannot imagine how the passage to which Mr. Lockyer refers ('Roy. Soc. Proc.,' vol. 32, p. 202) can be supposed to warrant it. The flame we mention in that passage is not that of a Bunsen burner but that of burning magnesium, which may be very different from the former even when the magnesium is burning in the air which is mixed with coal gas in the Bunsen burner. Moreover, whatever the temperature of the flame may be, we have never observed the triplet at $\lambda 3730$ unaccompanied by other ultra-violet lines. In the flame of burning magnesium, as we state (*loc. cit.*, p. 189), "photographs show, besides, the well-known triplet in the ultra-violet between the solar lines K and L sharply defined, and the line for which Cornu has found the wave-length 2850 very much expanded and strongly reversed."

We have expended a vast amount of time and trouble over vacuous tubes, and our later experiments do but confirm the opinion which we had previously formed that there is an uncertainty about them, their contents and condition, which makes us distrustful of conclusions which depend on them. Photographs of the ultra-violet spectra given by such tubes tell tales of impurities as unexpected as they are difficult to avoid. Every tube of hydrogen which we have examined exhibits the water spectrum more or less, even if metallic sodium has been heated in the tube or the gas dried by prolonged contact with phosphoric oxide. Indeed the only tubes which do not show the water spectrum have been filled with gases from anhydrous materials contained in a part of the tube itself; and even when tubes have been filled with carbonic acid gas from previously fused sodium carbonate and boracic anhydride the water spectrum is hardly ever

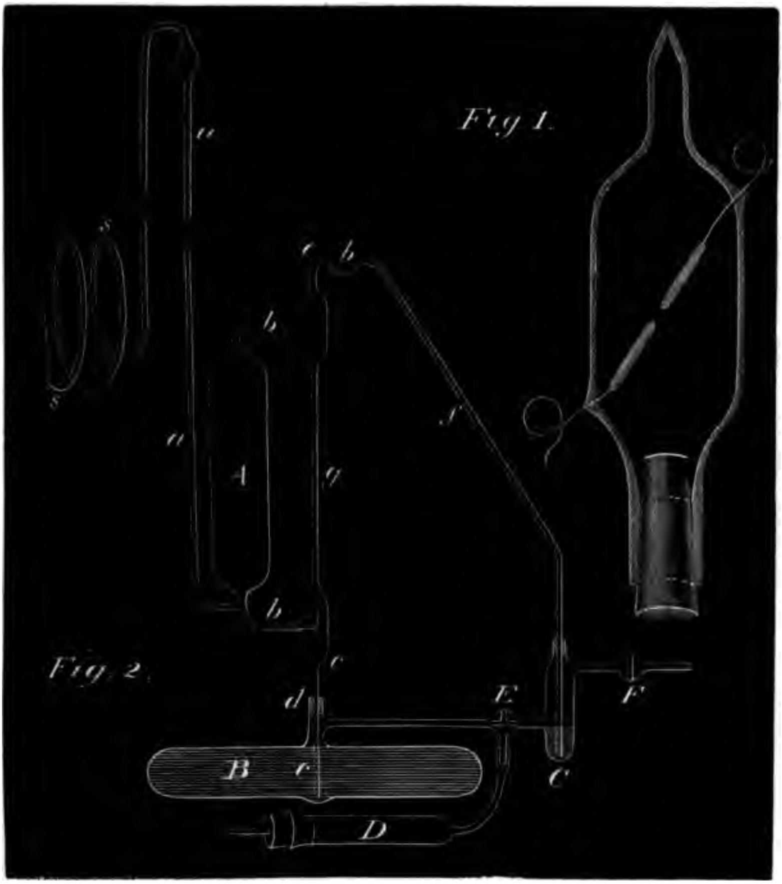
absent. The last traces of the ultra-violet bands of nitrogen are almost as difficult to be rid of with certainty. Frequently unknown lines or bands make their appearance, and the same tube will at different times exhibit wholly different spectra. This is especially the case with tubes of rarefied gases which oppose much resistance to the passage of the electric discharge such as oxygen.

It is no easy matter to prepare tubes for the observation of ultra-violet rays to which glass is opaque. Our plan is to fit a sort of stopper of quartz to an "end-on" tube (fig. 1). This stopper is a slightly conical piece of rock-crystal with the truncated ends of the cone ground plane and polished. It is first fitted to the tube by grinding and then cemented in with some vitreous substance more fusible than glass. Formerly we employed sodium metaphosphate which answered fairly, but latterly we have used fused silver nitrate which is easier to manipulate. In any case it is very difficult to prevent the tubes cracking under variation of temperature, but if the tube does not crack it is as effectually closed in this way as if it were all of one piece of glass. It is obvious that nitrogen, oxygen, and silver might be derived from silver nitrate used as cement and might add their spectra to those of the other contents of the tube. But the stopper does not lie in the direct course of the discharge, and we have not found that the silver nitrate is in general decomposed. The products of decomposition would at any rate give well-known spectra. The unknown and variable rays we are inclined rather to attribute to substances derived from the glass, either products of decomposition under the action of the electric discharge, or to matters adherent to the surface which become detached under some electric conditions, and adhere again when those conditions are changed. We have photographed the spectrum of one tube which had been filled with oxygen several times and exhausted, and which gave a well-marked spectrum containing a number of rays unknown to us. After a time other photographs of the same tube showed an entirely different spectrum, and after a further interval the spectrum was found to be again entirely changed, and finally after a further interval the original spectrum reappeared. Changes in the surface tension between the glass and some adherent film may in this case have facilitated the disengagement of the matter of the film and its after re-adherence. Whatever the cause, such changes of the spectra are none the less confusing and suggestive of caution in drawing our inferences from the phenomena of vacuous tubes.

The ultra-violet magnesium lines which we have observed in vacuous tubes with magnesium electrodes, when the induction coil, without jar, is employed, are the triplets at $\lambda 3837$, and the lines $\lambda$*2852, 2802, and 2795.* These appear whether the residual gas be *air, oxygen, hydrogen, or* carbonic acid. When a jar is used we have

obtained also the triplets at P and S, the pair about λ2935 and 2927, all the quadruple group near λ2802 and the quadruple group beyond, and *in one case only*, in oxygen, the group near *s*, described below, and the flame-triplet near M. When no jar is used sometimes only λ2852 is to be seen, sometimes λ2852 and the strong pair near λ2802, and sometimes also the triplet near L. We infer, therefore, that this is the order of persistency of these lines under the circumstances.

We have before remarked upon the necessity of avoiding all rubber connexions in the construction of pumps employed in the exhaustion of tubes for spectroscopic observation, and we described a modification of the Sprengel pump which we had constructed for this end ('Roy. Soc. Proc.,' vol. 30, p. 499). The warnings of unexpected impurities given by photographs of the ultra-violet spectra of vacuous tubes have shown the necessity of preventing the contact of the mercury employed with the dust and moisture of the atmosphere. Hence we have used in the experiments described in this paper a mercurial pump constructed wholly of glass, and in which the same mercury is used over and over again without being exposed to any unfiltered air. For this pump we are indebted to the ingenuity and skill in glass-blowing of Mr. Lennox of the Royal Institution. The annexed figure (2) represents its construction. A is a reservoir which communicates by the tube *aa*, which ascends vertically some distance in order to prevent any mercury being driven into the exhausted tube, through the spiral tube *ss*, with the tube to be exhausted. B is the reservoir of mercury, to the bottom of which the tube *gcc* passes through the sealed joint *d*. The upper part of B can be put in communication through the three-way cock E, either with the vessel C or with the outer air through the tube D which is filled with calcium chloride. C forms a mercury valve, and at its upper part communicates through the stopcock F with an exhaust pump by which the pressure of the gas in C can be quickly reduced to a few millimetres of mercury. When this has been done, the three-way cock E is turned so as to cut off the communication between B and C and open that between B and D. The pressure of the air filtered through D forces the mercury in B up the tube *c* until it fills A and the whole apparatus, as high as the bend *e*, driving all gas before it through the tube *f* and through the mercury valve C, whence it is carried off by the exhaust. The tube *g* is very narrow so as to oppose resistance to the passage of the mercury whereby A is filled with mercury as quickly as *g*. As soon as the last bubble of gas has been driven out of *f*, the three-way cock E is turned so as to shut the communication with D *and open* that between B and C. As the pressure of the air on the *surface* of the mercury in B diminishes the mercury falls both in A *and in f*, leaving a Torricellian vacuum above it, and, as soon as




has fallen below the end of the tube *a*, the gas in the tube to be exhausted expands into A. The same process is then gone through again and again, whereby the whole gaseous contents of A are each time removed, and if the volume of A be large compared with that of the tube to be exhausted, the pressure of the gas in the latter is very quickly reduced. The bends *bbb* retain a little mercury when A is exhausted, and prevent any diffusion from *c* into A, and from *f* into *c*. Each time the mercury fills the apparatus a small quantity flows over into C, but when it has risen above the opening of the tube connecting C and B, it passes back into B, when the cock E is turned so as to open the communication between C and B.

*Group near s.*

In their list of lines in the spectrum of magnesium ('Phil. Trans.,' 1884, p. 95) Messrs. Hartley and Adeney have given two lines, λ3071·6 and λ3046·0, which we had not heretofore observed either in the spectrum of the flame, arc, or spark of magnesium; but in our recent observations we have noticed in many cases a well-marked line which, by interpolation between neighbouring iron lines, appears to have a wave-length about 3073·5, and a pair of narrow bands sharply defined on their less refrangible sides at wave-lengths about 3050·6 and 3046·7, and fading away on their more refrangible sides.

We have little doubt that the lines we have observed are identical with those given by Messrs. Hartley and Adeney, notwithstanding that there is a much greater discrepancy between the wave-lengths assigned by them and by us than there is between the wave-lengths we have respectively found for the iron lines in the same neighbourhood.

We have noticed the occurrence of this group in the spectrum of the arc from a Siemens' dynamo between magnesium electrodes in a variety of gases, in all in fact in which we have examined the arc except in sulphurous acid gas which is opaque to rays of this refrangibility. Also in the arc from a De Meritens' magneto-electric machine between magnesium electrodes in air, in the flash of a magnesium ribbon dissipated by the discharge of a storage battery, in the spark of an induction coil worked in the usual way in air and in hydrogen at atmospheric pressure, and in one instance in the spectrum of an oxygen vacuous tube with magnesium electrodes when a Leyden jar was connected with the secondary wire of the induction coil.

On the other hand, we do not see this group in the spectrum of other vacuous tubes with magnesium electrodes or with magnesia in the tube, nor in the spark from an induction coil in air or hydrogen at atmospheric pressure when the coil is worked with a De Meritens' machine on the primary wire, nor in the flame of burning magnesium, nor in the oxyhydrogen flame with magnesia or magnesium chloride, nor in the arc between carbon electrodes in a crucible of magnesia.

The circumstances under which this group is seen and is not seen, do not seem to indicate that its emission is connected with any particular temperatures so much as with the character of the electric discharge, and perhaps also with the density of the magnesium vapour.

*Presents, May* 31, 1888.

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## "On the Coagulation of the Blood." Preliminary Communication. By W. D. HALLIBURTON, M.D., B.Sc., Assistant Professor of Physiology, University College, London. Communicated by Professor E. A. SCHÄFER, F.R.S. (From the Physiological Laboratory, University College, London.) Received March 20,—Read April, 26, 1888.

The theory to account for the coagulation of the blood which is most generally accepted at the present day is that of Hammarsten; he teaches that coagulation is dependent upon the conversion of a proteid substance, fibrinogen, which exists in solution in the plasma, into fibrin by means of a ferment liberated by the disintegration of the white blood corpuscles which occurs when the blood leaves the living blood-vessels. This theory has replaced the older one of Al. Schmidt, who taught that fibrin is formed by the union of two fibrin-generators, one of which is the fibrinogen just mentioned, and the other of which he called fibrinoplastic substance or paraglobulin; this union, moreover, occurs under the influence of a third factor, the fibrin ferment.* Hammarsten† showed that paraglobulin, or as it is now more generally called serum globulin, is not necessary for the formation of fibrin.

The present research was directed to determining the nature of the ferment that produces this change in fibrinogen. The result at which I have arrived is sufficiently definite to warrant a preliminary statement of the facts observed; the full details of the experiments, as well as those of certain others which are at present in progress, will be reserved for a later communication.

I will first briefly relate some preliminary experiments‡ which had

* 'Pflüger's Archiv,' vol. 6, p. 413 *et seq.*

† *Ibid., vol. 14, p. 211; 17, p. 413;* 18, p. 38; 19, p. 563.

‡ *An account of some of these preliminary* experiments is contained in the report

for their object a separation and recognition of the various proteids contained in lymph cells. An animal (generally a cat) was chloroformed and killed by bleeding from the carotids; the thorax was quickly opened, and a cannula inserted in the aorta; a stream of salt solution (¾ per cent.) at considerable pressure was passed through the vessels by this means, in about a minute the large veins entering the heart were opened and the mixture of blood and saline solution allowed to escape. When the fluid came through perfectly colourless, the abdominal glands were removed, freed from their capsules, cut into small pieces, and ground up in a mortar with saline solution; any portions of the gland capsules which still remained were removed, and the fluid with the cells suspended in it was poured into test-tubes, the cells settled, and the process of settling was hastened by centrifugalising, the supernatant liquid was poured off, and the cells again washed with saline solution in the same way. By this method the cells were quickly freed from any lymph that might still have been in contact with them.

Microscopical examination showed that they still possessed their normal appearances, except for a small amount of shrinkage. The supernatant saline liquid was found to contain in small quantities the proteids which were afterwards found in the cells, a certain amount of their proteid constituents having thus entered into solution.

The liquid which was found best for dissolving the proteids of the lymph cells thus obtained was prepared by mixing a saturated solution of magnesium sulphate with nine times its volume of distilled water; and the proteids present in such an extract were as follows:—

1. A mucin-like proteid similar to that described by Miescher* in pus which swells up into a jelly-like substance when mixed with solutions of sodium chloride or magnesium sulphate.

2. Two globulins.

3. An albumin.

It will be convenient to take these proteids one by one, and describe the chief properties of each.

1. *The Mucin-like Proteid.*—If the cells are extracted with a 5 per cent. sodium chloride or magnesium sulphate solution, the result is a slimy mass, resembling mucus in appearance. The proteid which causes this appearance may be obtained pure by pouring this mixture into a large excess of distilled water; this peculiar proteid then extends in cohesive strings throughout the water, which in time contract and float on the top, and may be then thoroughly washed with distilled water. The following are its chief properties; it is insoluble in water, slightly soluble in ¾ per cent. saline solution, as shown by

*of a committee* appointed by the British Association to investigate the physiology of *the lymphatic system* ('Brit. Assoc. Rep.,' 1887, p. 145).

* *Hoppe-Seyler's* 'Med. Chem. Untersuchungen,' p. 441.

the fact that such a solution becomes slimy when the proportion of salt is increased to 5 per cent. It is also slightly soluble in the sodium sulphate solution used. When this proteid is suspended in water or salt solution the mucus-like strings shrink at about 50° C., and can be easily filtered off. In the case of sodium sulphate extracts of the glands, it is apparently carried down with the globulin that coagulates at that temperature. Saturation with neutral salts, sodium chloride, magnesium sulphate, and especially ammonium sulphate, causes also shrinkage of the swollen masses, and renders filtration easier. It is precipitable by absolute alcohol, basic lead acetate, and by solution of tannin. It is precipitated by acetic acid in strings like mucin; like mucin also it is soluble in baryta or lime-water, from which solution it is again precipitable by acetic acid, and only soluble in considerable excess of that reagent.

This substance, however, is not mucin, as prolonged boiling with sulphuric acid does not cause it to yield any reducing sugar. It is also not nuclein of which the cell nuclei are made up, as the nuclei are not attacked by such reagents as $\frac{3}{4}$ per cent. sodium chloride in which this substance is slightly soluble. It, however, like nuclein, yields an ash which is rich in phosphorus; it dissolves in 0·2 per cent. hydrochloric acid, and on adding pepsin to this solution an insoluble residue rich in phosphorus separates out. Otherwise this substance has the nature of a globulin, but one which is much more readily precipitated by neutral salts than most globulins are; a proportion of 5 per cent. of sodium chloride for instance in its solutions rendering it insoluble; but the precipitate so produced is not of the usual fine flocculent character, but a slimy mucus-like one. In all these points this proteid resembles in its characters a class of proteids which have been recently named "nucleo-albumins" by Hammarsten.* He has separated these mucin-like globulins from the bile, and from synovial fluid where they have long been mistaken for mucin, and from the cells of the submaxillary gland, which contain, however, true mucin in addition.

2. *The Globulins.*—There is a small quantity of a globulin which enters into the condition of a heat coagulum at about 50° C. The most abundant globulin is, however, one which resembles serum globulin in its heat coagulation temperature (75° C.), and in the way in which it is precipitated by saturation with salts, or by dialysing out the salts from its solutions.

The term serum globulin is hardly applicable to a proteid existing in lymph cells; hence it is necessary to multiply terms, and to designate this globulin by a new name, viz., *cell globulin*. It has, moreover, certain characteristic properties which will be fully dealt with later on.

* 'Zeitschr. Physiol. Chem.,' vol. 12, p. 163.

3. *The Albumin* resembles serum albumin in its properties. It coagulates at 73° C. It is present in very small quantities. It may be provisionally termed *cell albumin*.

In concluding this account of the proteids of lymph cells, I may add that no substance like myosin or fibrin can be obtained from the cells; there is, however, a formation of sarkolactic acid after death as in muscle; and if the glands be left, especially at the temperature of the body, for some hours after death, a process of self-digestion takes place, the pepsin present in the glands as it is in most tissues (Brücke) becoming active when the reaction of the tissue becomes acid; under these circumstances there is in addition to the proteids already enumerated a small and varying amount of albumoses and peptones.

Having thus recognised the various proteids that occur in the cells of lymphatic glands, my next endeavour was to ascertain what action, if any, these exerted on the coagulation of the blood. My experiments in this direction have been mostly performed with salted plasma. The blood is received into an approximately equal volume of saturated sodium sulphate solution. By this means coagulation is prevented, and the corpuscles settle. On subsequently removing the supernatant salted plasma, and diluting it with four or five times its bulk of water, coagulation occurs after the lapse usually of several hours; but if, instead of water, a solution of fibrin ferment be used, coagulation occurs in a few minutes.

I first tried to prepare fibrin ferment from the lymphatic glands; these were freed from blood, chopped small, and placed under absolute alcohol for some months; they were then dried over sulphuric acid, powdered, and the dry powder extracted with water. The water was found to contain the fibrin ferment. It hastened very considerably the coagulation of salted plasma. This activity was destroyed at a temperature between 74° C. and 80° C. The watery extract gave, moreover, the xanthoproteic reaction; it contained also some sodium chloride and phosphates which it had dissolved out of the dried glands.

A watery or saline extract of fresh glands also had very considerable clotting powers;* that is to say, the addition of a few drops of such an extract caused diluted salted plasma to clot in a few minutes, which otherwise did not clot until after the lapse of 12—24 hours. The activity of this extract was not altered by heating to 70°; it was therefore independent of the nucleo-albumin which is disintegrated at about 50° C., or of the globulin which coagulates at that temperature. Its activity was destroyed, however, if heated above 75° C. These facts show that the extracts of both dried and fresh glands *contain a substance* which has the same properties as fibrin ferment,

* *I find that this* fact has been previously noted by Rauschenbach, 'Inaug.-*Dissert.*,' Dorpat, 1882, p. 26.

nd which, moreover, is rendered inactive at the temperature at 'hich fibrin ferment, as ordinarily prepared from serum, loses its ctivity.

The next question which I investigated was whether the ferment :tion was dependent upon, or independent of, the presence of the roteids of the cells. An extract of the cells was made with sodium ılphate solution, and saturated with ammonium sulphate; the pre'pitate of the proteids so produced was filtered off; the proteid-free ltrate dialysed till free from excess of salt,* and it was then found › have no power of hastening coagulation. The precipitate which ›ntained all the proteids was washed by saturated solution of ammoium sulphate, and redissolved by adding distilled water (Solution A); ıis solution hastened the coagulation of salted plasma very conderably. This experiment showed either that the ferment was .entical with or precipitated with the proteids in the extract. It as, moreover, destroyed at a temperature at which these proteids ere coagulated, viz., about 75° C.; there are, however, in Solution A wo proteids which are coagulated at about this temperature, viz., ıe cell globulin and the cell albumin. These were separated by ıturating the solution with magnesium sulphate; the globulin was recipitated, washed, and redissolved by adding water (Solution B). 'he filtrate from this precipitate was dialysed till free from salt Solution C). Solution B was dialysed until nearly free from salt, ›ut not sufficiently free to cause precipitation of the globulin; it was divided into two equal parts, B′ and B″; B′ underwent no further treatment. B″ was dialysed till the globulin was precipitated; the globulin was then filtered off, washed with distilled water, the precipitate dissolved in 0·3 per cent. sodium chloride solution (Solution D). The solution B″ *minus* the globulin precipitated by dialysis still contained a small quantity of globulin; this may be called Solution E.

The influence of each of these solutions on dilute salted plasma was hen investigated. The results may be summarised as follows:—

*Solution C* (containing only cell albumin) did not hasten the coagution of salted plasma, but in some cases even caused delay.

*Solution B′* (containing only cell globulin) hastened very considerably the coagulation of such plasma.

*Solution E* (containing very little cell globulin) hastened the coagution to a slight extent.

*Solution D* (containing the cell globulin precipitated from Soluon B by dialysis) hastened the coagulation considerably.

These experiments show that it is not the albumin but the globulin

* This experiment, and the others in which dialysis was employed, were carried t in the cold winter months, and thymol was always added to prevent putreion.

which has the properties of fibrin ferment. It might be urged that the ferment is not identical with the globulin, but is only closely associated with it. Such an objection seems to me to be a mere splitting of hairs. If the ferment is so closely associated with the globulin that none of the methods used of preparing the globulin pure are capable of separating it from the ferment, and if, moreover, the activity of the ferment is destroyed when the distinctive characters of the globulin are destroyed, as by heating to 75° C., then we are not justified in saying that the globulin is different from the ferment, until some method is shown by which they may be separated.

After I had performed the experiments just related, the question naturally arose, is this cell globulin the same thing as what has been termed fibrin ferment when prepared from serum? The experiments that I performed in attempting to find an answer to this question were as follows:—

A large quantity of cat's serum was taken, and to it was added 10 to 15 times its volume of absolute alcohol. The resulting precipitate was allowed to stand under the alcohol for about three months; the alcohol was then filtered off, and the precipitate dried over sulphuric acid and powdered. On extracting this powder with water, especially with warm water, a very active preparation of fibrin ferment was obtained. Like all preparations of the fibrin ferment, it gave the xanthoproteic reaction, but sufficient proteid was not present to enable one to identify it. The extract was therefore concentrated at 40° C.; it was then found to contain a proteid which was coagulated by heat at 75° C. It was precipitated by dialysing out the salts from its solutions, and it was also precipitated by saturation with magnesium sulphate;* the precipitate produced by magnesium sulphate was collected, washed with a saturated solution of magnesium sulphate, and redissolved by the addition of water, the adherent salt rendering it soluble. This solution has very marked ferment properties; it hastened the coagulation of salted plasma; it caused pericardial fluid to clot rapidly; and it also hastened the coagulation of pure plasma obtained from the jugular vein of the horse. This last-mentioned experiment is of especial importance, as here the plasma was unmixed with any foreign substance. The jugular vein of a horse was removed after being ligatured in two places to prevent the blood escaping; the "living test-tube" was suspended in a cold place over night, and in the morning the corpuscles had subsided; the plasma above these was almost free from corpuscles; and when removed from top of the vein by a pipette did not clot for about half an hour at the temperature of the air (11° C.); but a similar portion to which a few drops of

* After filtering off the precipitate produced by magnesium sulphate, the filtrate contained the merest trace of proteid, and on dialysing away the excess of salt, it was found to have lost all the properties of fibrin ferment.

the ferment globulin had been added coagulated in about two minutes.

The question will be asked, how is it if the ferment is a globulin it can be extracted by means of distilled water from the ferment powder? The answer to that question is that the water is enabled to dissolve the globulin by a portion of the salts, especially sodium chloride, in the ferment powder entering into solution at the same time. That this is the correct answer was shown by the following experiment:—A quantity of the ferment powder was subjected to prolonged washing with warm (40° C.) distilled water; it was then suspended in water, and dialysed for three weeks, thymol being added to prevent decomposition. At the end of this time it was dried over sulphuric acid; it was then found that warm water was able to extract only the faintest trace of proteid from the powder, and that this extract had little or no ferment action, while an extract of the same powder with a 0·3 per cent. sodium chloride solution contained much more proteid and had powerful ferment properties.

Serum globulin prepared from sheep's and horse's serum by repeated precipitation with magnesium sulphate, and finally by dialysis, was found to possess powerful ferment properties; this entirely confirms Al. Schmidt's statement that he has been unable to prepare from serum "fibrinoplastic substance" free from ferment.* This is easily explained when one considers that serum globulin as prepared from serum contains a certain admixture of cell globulin derived from the disintegration of white blood corpuscles; and this is precipitated with the globulin which pre-existed in the blood plasma. On the other hand, serum globulin prepared from a liquid like hydrocele fluid which does not coagulate spontaneously, has no such ferment properties. This confirms Hammarsten's statement that he has obtained from hydrocele fluid a pure paraglobulin free from ferment, and which exerted no fibrinoplastic activity.

I will here quote a typical experiment which brings out the fibrinoplastic properties of globulin prepared from serum, and the absence of such properties in the globulin prepared from hydrocele fluid:—

Ox sodium sulphate plasma was diluted with four times its volume of liquid in each of the succeeding experiments; the diluted plasma was then divided into two parts, one part was kept at the temperature of the air (14° C.), the other at the temperature of 40° C. in an incubator.

Thus the plasma which was diluted with a saline solution of globulin from hydrocele fluid, coagulated at approximately the same time as that in which the saline solution alone was employed as the diluent,

* I have also confirmed Schmidt's statement that serum globulin (Schmidt's *fibrinoplastic substance) which is* precipitated by a stream of carbonic acid, has *ferment properties.*

| Dilution with | Coagulation occurred | |
|---|---|---|
| | At 14° C. | At 40° C. |
| 1. 0·3 per cent. NaCl solution.......... | In 46 minutes | In 20 minutes |
| 2. Globulin from horse serum dissolved 0·3 per cent. NaCl solution........ | „ 10 „ | „ 2 „ |
| 3. Globulin from hydrocele fluid in 0·3 per cent. NaCl solution .......... | „ 47 „ | „ 22 „ |

while a specimen diluted with a saline solution of globulin from seri coagulated in about one-fifth of that time. The explanation of t difference in the action of the serum globulin as derived from th two sources is perfectly clear in the light of the foregoing researcl in the ferment powers of cell globulin.

The serum globulin from hydrocele fluid contains no ferme because it contains pure serum globulin and no cell globulin.

The serum globulin obtained from serum contains in addition the serum globulin that existed in the blood plasma, a certain quant of cell globulin formed by the disintegration of white corpuscles.

Both Schmidt and Hammarsten have recognised the fact that t amount of globulin in the serum was greater than in the plasma, a that this extra amount was derived from the white blood corpuscl The object of this paper is to point out that this extra globulin deriv from the white corpuscles is in reality fibrin ferment. I may h mention that examination of the ash of this substance shows that contains no phosphorus.

Preparations of serum from which the globulin had been remov by saturation with magnesium sulphate, and the excess of salt dialysis, were found to have no ferment activity at all. Schmi found that serum *minus* its globulin (precipitated by a stream of ca bonic anhydride) has very little ferment activity; the explanation its still possessing any is that carbonic acid does not completely pr cipitate the globulin. When, however, the globulin is complete removed by magnesium sulphate, all ferment activity is complete removed also.

An extract of "washed blood clot" was found by Buchanan* hasten the formation of fibrin. Gamgee,† on repeating Buchanar experiments, concluded that the substance in saline extracts of fibr which had the powers of fibrin ferment was a globulin; and this vie entirely coincides with the conclusions I have arrived at. In a fe experiments in which I have used a 5 per cent. magnesium sulpha extract of fibrin, I obtained in the extract a globulin which has

* 'London Medical Gazette,' vol. 18, p. 50.

† 'Journal of Physiology,' 1879.

the properties of fibrin ferment, which coagulates at 75° C., and agrees in all other particulars with the substance I have named cell globulin. It is derived doubtless from the white corpuscles entangled in the clot. Lea and Green,* who repeated Gamgee's experiments, came to somewhat opposite conclusions; they, however, never obtained the ferment free from proteid, but they concluded it was not a globulin as it was soluble in distilled water; they admitted, however, that it was much more soluble in saline solutions; reading these experiments in the light of a more recent paper by one of them,† it is evident that they were dealing in large measure with calcium sulphate, a salt which has considerable powers of aiding the activity of the fibrin ferment.

The final conclusions that are to be drawn from these researches are as follows:—

1. Lymph cells yield as one of their disintegration products a globulin which may be called cell globulin. This has the properties that have hitherto been ascribed to fibrin ferment.

2. Fibrin ferment as extracted from the dried alcoholic precipitate of blood serum is found on concentration to be a globulin with the properties of cell globulin.

3. The fibrin ferment as extracted by saline solutions from "washed blood clot" is a globulin which is also identical with cell globulin.

4. Serum globulin as prepared from hydrocele fluid has no fibrinoplastic properties. It may perhaps better be termed plasma globulin.

5. Serum globulin as prepared from serum has marked fibrinoplastic properties. This is because it consists of plasma globulin, and cell globulin derived from the disintegration of white blood corpuscles, which are in origin lymph cells.

6. The cause of the coagulation of the blood is primarily the disintegration of the white blood corpuscles; they liberate cell globulin which acts as a ferment converting fibrinogen into fibrin. It does not apparently become a constituent part of the fibrin formed.

This confirmation and amplification of Hammarsten's views concerning the cause of the coagulation of the blood is in direct opposition to the theories of Wooldridge. My methods have not been the same as those adopted by Wooldridge, but the final conclusions are so different, that it is necessary I should state my reasons for not accepting his views, nor adopting his methods. Wooldridge's theory may be stated as follows:‡—The coagulation of the blood is a phenomenon essentially similar to crystallisation; in the plasma there are three constituents concerned in coagulation, A, B, and C fibrinogen.

* '*Journ. of Physiol.*,' vol. 4, p. 380.

† *Green*, '*Journ.* of Physiol.,' vol. 8, p. 355.

‡ *Croonian Lecture*, Royal Society, 1886.

A and B fibrinogen are compounds of lecithin and proteid, and fibrin results from the transference of the lecithin from A fibrinogen to B fibrinogen. C fibrinogen is what has hitherto been called fibrinogen; A fibrinogen is a substance which may be precipitated by cooling "peptone plasma," and on the removal of this substance coagulation occurs with great difficulty. The precipitate produced by cold consists of rounded bodies resembling the blood-plates in appearance. He further found that other compounds of lecithin and proteid to which he has extended the name of fibrinogen exist in the testis, thymus, and other organs, in the fluid of lymph glands, and in the stromata of red corpuscles; these substances may be extracted from the organs by water, and precipitated from the aqueous extract by acetic acid, and on redissolving this in a saline solution, and injecting it into the circulation of a living animal, intravascular clotting occurs which results in the death of the animal.* This form of fibrinogen (?) that acts thus he looks upon as the precursor of A fibrinogen. From these points of view the fibrin ferment and the white corpuscles are looked upon as of secondary import in causing coagulation, though it is admitted that fibrin ferment converts C fibrinogen into fibrin.

In a more recent paper* these fibrinogens are somewhat differently lettered; B fibrinogen seems to have disappeared, and C fibrinogen now receives that name.

I have been carefully through all Wooldridge's papers, and I have by examination of the statements made therein, and by a few test experiments of my own, come to the conclusion that the theory is untenable; I will take up the chief facts upon which the theory rests, one by one.

1. *The Influence of Lecithin in the Coagulation of the Blood.*—Lecithin hastens the coagulation of blood-plasma, which has been prevented from clotting by the injection into the circulation of a certain quantity of commercial peptone.† The term "peptone plasma" is a convenient one to retain, though it must be remembered that it is not the peptone in it that has the action in question, but the albumoses, and especially heteroalbumose.‡ Wooldridge§ also found by receiving the blood of a dog into a thick emulsion of lecithin coagulation occurred more quickly than when it was received into a corresponding quantity of saline solution.

It is upon these experiments that the theory that lecithin is the essential cause of the coagulation depends. I am very little inclined

* Ludwig's 'Festschrift,' p. 221.

† Wooldridge, 'Journ. of Physiol.,' vol. 4, p. 226.

‡ Pollitzer, 'Journ. of Physiol.,' vol. 7, p. 289. Pollitzer also found that these proteids also delayed the coagulation of blood after it was shed. I have found them to cause a similar delay in the clotting of dilute salted plasma.

§ 'Journ. of Physiol.,' vol. 4, p. 367.

to place reliance on the experiments on dog's blood just quoted, as he finds it necessary to use an emulsion as thick as milk to produce the effect, and it is well known that contact with any foreign matter, especially if it is finely divided, will hasten coagulation, and it cannot be supposed that sufficient lecithin is normally concerned in forming fibrin as to cause a thick emulsion like this. Moreover, addition of lecithin does not cause the clotting of pericardial fluid, of hydrocele fluid, of solutions of fibrinogen,* of dilute salted plasma,† and I am not aware that it has been tried on pure plasma obtained by the living test-tube experiment. It then simply hastens the coagulation of peptone plasma, and peptone plasma, as I shall show more fully in the next section, differs so much from normal plasma, that it is impossible to draw correct conclusions from experiments performed with it, unless they be supported by confirmatory evidence on solutions of fibrinogen and pure plasma, such as one obtains from a vein, or from the pericardial sac.

The solutions of lecithin used were admittedly impure,‡ and it is possible that there was present a certain amount of calcium sulphate, even if there was no fibrin ferment. But supposing it was the lecithin and not the impurities that hastened the coagulation in question, it must be remembered that many other organic and inorganic substances act similarly; thus Nauck§ has shown that small quantities of glycin, uric acid, &c., as well as lecithin hasten coagulation, and Green‖ that calcium sulphate does so also. But it is not concluded from these observations that these are the chief agents in bringing about the coagulation of the blood. I have found that cell globulin contains no phosphorus, and Wooldridge admits¶ that Schmidt's ferment is free from lecithin. On the very same page, however, he accounts for the loss of the activity of fibrin ferment, which was observed to take place by Hammarsten when it was kept long under alcohol, as being due to removal of lecithin.**

The supposition that "fibrinogen A" acts by giving up its lecithin to "fibrinogen B" to form fibrin, seems, therefore, to be a pure assumption, and so far as I can find is unsupported by any analytical evidence. Wooldridge has certainly shown that the fibrinogens (?) he obtains from tissues contain phosphorus, but to this point I shall return later.

* 'Journ. of Physiol.,' vol. 4, p. 369.

† Private communication to the author.

‡ 'Journ. of Physiol.,' vol. 4, p. 369.

§ 'Inaug.-Dissert.,' Dorpat, 1886.

‖ *Loc. cit.*

¶ 'Journ. of Physiol.,' p. 230.

** This loss of activity is well explained by my theory, by supposing that the longer cell globulin is kept under alcohol, the more insoluble in water does it become, like all other proteids.

2. *The Precipitate produced by cooling Peptone Plasma* (Wooldridge's fibrinogen A).—The occurrence of this precipitate is evidently regarded by Wooldridge as one of the most important facts upon which his theory is founded. Here, again, I am willing to concede the fact observed, but differ from Wooldridge with regard to its interpretation. The chief point I wish to urge is that this precipitate is obtained on cooling peptone plasma only, and from no other form of plasma. I have repeatedly attempted to obtain such a precipitate by cooling to 0° C. pure plasma from the veins of the horse,* salted plasma, prepared by mixing blood with various proportions of different salts, hydrocele fluid, and pericardial fluid, but in all cases with a negative result. It, therefore, occurs in peptone plasma alone; and that it is due to the peptone is supported by the fact that if one takes an aqueous solution of "Witte's peptone" and cools it to 0° C., a precipitate is formed consisting of rounded granules, which were mistaken under the microscope by several friends in the laboratory for blood-tablets. I, moreover, found that this precipitate consists of heteroalbumose, and when that substance (which as Neumeister† has shown is more soluble in water than has been hitherto supposed) has been removed by dialysis and filtration, the remaining albumoses and peptones are not precipitated by cold. The precipitate of heteroalbumose, which is obtained by dialysis of saline solutions of "Witte's peptone," also consists of similar rounded granules.

Peptone plasma, it may be said, does not contain peptone or albumoses; or rather that it is difficult to discover them in "peptonised blood." It is undoubtedly difficult, because they are present in such small proportion that they are obscured by the overwhelmingly large amount of globulin and albumin present. But as Neumeister‡ has shown that they are, after injection into the circulation, excreted by the kidneys, we must also conclude that they exist as such for a time in the blood.

How their presence there prevents coagulation it is difficult to say; it is possible that they may cause by their presence a change in the normal proteids of the blood that prevents the formation of or the action of the fibrin ferment. That peptone blood does differ in one other important particular from normal blood, viz., in the heat coagulation temperatures of its proteids, was shown by Wooldridge

* I have found that if the plasma is completely frozen, that on subsequently thawing it, coagulation sets in very quickly (in 10—20 seconds); this is probably due to the crystals of ice breaking up the white corpuscles, many of which still float in the plasma. Nauck also has noted this and gives a similar explanation (*loc. cit.*, p. 20). After removing these by centrifugalising for two hours in vessels surrounded by ice, no such phenomenon occurs, and clotting does not set in for fully fifteen minutes after thawing.

† 'Zeitschr. Biol.,' vol. 24, p. 269.

‡ *Ibid.*, vol. 24, p. 281 *et seq.*

himself.* It is on these grounds, then, that I hold we cannot regard peptone plasma as being at all comparable to normal plasma.

With the removal of "fibrinogen A" the whole complex theory as formulated by Wooldridge falls to the ground; and we are left with "fibrinogen B" of the later communication, which is Hammarsten's fibrinogen. It is advisable to confine strictly the use of the term "fibrinogen" to this substance.

3. *Intravascular Coagulation.*—Under this heading my remarks will be of the nature of criticism only. No doubt the crude and impure substance (for there is no attempt at purification, separation, or identification) introduced into the veins produces intravascular clotting; but I must protest against the extension of the name fibrinogen to such substances. It seems to me it would be just as correct to call a piece of iron wire introduced into the sac of an aneurysm to produce coagulation there, a fibrinogen.

Some of Wooldridge's experiments under this head have been repeated by Krüger;† he finds, in opposition to Wooldridge, that leucocytes themselves produce intravascular clotting (which would agree perfectly well with the cell globulin theory), and also that the stromata of red corpuscles, which probably contain the same constituents in great measure as the white corpuscles, act similarly; other experiments have led him to the conclusion that it is the corpuscular elements that play the chief part in the coagulation, both within and without the body. He entirely negatives the statement of Wooldridge that the fluid of the lymph gland produces this effect, and any slight action it may have is accounted for by the presence of some leucocytes, which are exceedingly difficult to remove completely, even by centrifugalising.

To return, however, to these tissue fibrinogens of Wooldridge, I think we may venture to offer a suggestion as to their real nature, or, at any rate, as to the nature of one of their constituents. From the last paper published by Wooldridge,‡ we find that they are imperfectly soluble in water, readily precipitated by acids, and soluble in excess of those reagents. That they yield on gastric digestion a substance which is insoluble and which is rich in phosphorus. From these details of their properties, I think, we may draw the conclusion not that they contain lecithin, as Wooldridge affirms, but that they belong to the group of proteids described in the former part of this paper under Hammarsten's name of nucleo-albumin. Nucleo-albumins yield when poured into water a stringy precipitate resembling mucin, and in a former paper Wooldridge§ speaks of the preci-

* 'Roy. Soc. Proc.,' vol. 38, 1885, p. 263.
† 'Zeitschr. Biol.,' vol. 24, p. 189 *et seq.*
‡ '*Roy. Soc. Proc.*,' vol. 43, 1888, p. 367.
§ *Ibid.*, vol. 40, 1886, p. 134.

pitate of his tissue fibrinogen (precipitated by acetic acid) as being a bulky one. If my conjecture is correct, it would be exceedingly likely that when a saline solution of such a substance was injected into the circulation, it would form strings of a slimy mucinoid description in the vessels, and that these would form the starting-point for the thrombosis or intravascular coagulation that ensues.

*June* 7, 1888.

The Annual Meeting for the Election of Fellows was held this day.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Sir William Bowman and Dr. Gladstone were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Andrews, Thomas, F.R.S.E.
Bottomley, James Thomson, M.A.
Boys, Charles Vernon.
Church, Arthur Herbert, M.A.
Greenhill, Professor Alfred George, M.A.
Jervois, Sir William Francis Drummond, Lieut.-Gen. R.E.
Lapworth, Professor Charles, LL.D.
Parker, Professor T. Jeffery.
Poynting, Professor John Henry, M.A.
Ramsay, Professor William, Ph.D.
Teale, Thomas Pridgin, F.R.C.S.
Topley, William, F.G.S.
Trimen, Henry, M.B.
Ward, Professor Henry Marshall, M.A.
White, William Henry, M.I.C.E.

*Re-elected.*

Clarke, Alexander Ross, Colonel R.E.

Thanks were given to the Scrutators.

*June* 7, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

## I.* "Note on some of the Motor Functions of certain Cranial Nerves (V, VII, IX, X, XI, XII), and of the three first Cervical Nerves, in the Monkey (*Macacus sinicus*)." By CHARLES E. BEEVOR, M.D., F.R.C.P., and VICTOR HORSLEY, B.S., F.R.S. (From the Laboratory of the Brown Institution). Received May 16, 1888.

In the course of an investigation which we are making into the cortical representation of the muscles of the mouth and throat, we have experienced considerable difficulty in describing correctly the movements of these parts, especially when there was any question of bilateral action occurring.

On referring to text-books we failed to find any solution of this difficulty, and we therefore determined to make a few observations of the movements evoked by stimulating the several cranial nerves supplying this region in the monkey† so as to have a definite basis whereon to ground our observations of the movements obtained by stimulating the cortex.

In the course of this work we have observed several facts which do not harmonise with the views hitherto generally received.

The results are summarised as follows :—

### *Method of Investigation.*

The foregoing summary of our experiments is based almost entirely upon the results obtained by exciting the respective nerves at the base of the cranial cavity after separating them from the bulb.

We have also stimulated the nerves outside the skull in the neck both before and after division.

* Towards the expenses of this research a grant was made by the British Medical Association, on the recommendation of the Scientific Grant Committee of the Association.

† Previous observers having employed animals of lower orders.

In every case the animal was narcotised with ether.

(1.) For the exposure of the nerves at the base of the cranial cavity it was found possible to rapidly remove a cerebral hemisphere, clamping the carotid and other arteries, then to divide the tentorium and to remove the major part of the cerebellar hemisphere of the same side, so as to admit of prolonged and numerous observations before the animal died. In all we have made eight experiments, and in every case we have operated on the same kind of monkey, *i.e.*, *Macacus sinicus*.

(2.) For the exposure of the nerves outside the skull we found it easy to lay bare the upper cervical nerves and those of the cranial division in the anterior triangle by turning forward a triangular flap of skin, ligaturing and removing the external jugular vein, and dividing and turning aside completely the sternomastoid muscle. Finally, the parotid gland and digastric muscle (posterior belly) were drawn up with hooks, the head being turned to the opposite side.

The chorda tympani was readily exposed without injury, in the tympanic cavity, *before* the dissection of the triangle by cutting away the posterior wall of the external auditory meatus and the posterior half of the tympanic ring. The facial nerve was subsequently exposed in the stylomastoid foramen and aqueduct.

The nerves were in each case raised up from their position and stimulated in the air by the faradic current through fine platinum electrodes, the area of the operation having been gently dried.

The current employed was from the secondary coil of an ordinary du Bois-Reymond inductorium, supplied by a 1 litre bichromate cell. The experiment was carefully begun with the secondary coil at a distance of 30 cm. from the primary, this interval being very rarely diminished to more than 15 cm. (zero being of course the point where the secondary coil completely overlaps the primary).

### *Further Observations respecting the Examination of each Nerve.*

#### A. *Cranial Division.*

*Vth Nerve.*—Excitation of the motor root of the trigeminus evoked powerful closure of the jaws, and although the muscles of one side only were in action, the teeth were approximated without any lateral deviation of the lower jaw.

*VIIth Nerve.*—The motor distribution of the facial nerve has for the most part been well known for some time. However, we consider that, unfortunately, a very fundamental error respecting this distribution has crept into the text-books, it being supported by one anatomical authority following another, and, moreover, having been accepted by clinicians as an important aid in the differential diagnosis of facial paralysis. We refer to the supposed supply of motor fibres

from the facial to the levator palati through the superficial petrosal nerve.

This idea,* upon which so much stress has been laid, is entirely hypothetical, as might have been shown at any time by stimulating the facial nerve in the skull, and observing the soft palate.

We have found that stimulation of the peripheral end of the divided facial nerve in the internal auditory meatus failed to cause even with most powerful currents the slightest movement of the soft palate, although the face was thrown into violent spasm. The true motor nerve supply of the levator palati is, according to our observations, the XIth nerve (*vide infra*).

*IXth Nerve. Glossopharyngeal.*—In exciting this nerve, in addition to the movements of the pharynx, which we attribute to the contraction of the stylopharyngeus, and possibly to the middle constrictor of the pharynx, we have observed certain movements of the palate, as follows:—(I.) Stimulation of the nerve while beneath the stylohyoid ligament and uncut, gave in two instances elevation of the palate on the same side, and in one instance on both sides. We suppose that everyone will consider with us this movement to be reflex in origin, but we must add (II) that in one case we saw elevation of the palate to the same side when exciting the peripheral end of the cut nerve. In this latter case, perhaps, the result may be explained by the close neighbourhood of the pharyngeal plexus and the possible escape of current thereto, and under any circumstances this is but a single exceptional observation, so that we lay no stress upon it. Finally we never saw movement of the soft palate when the glossopharyngeal nerve was stimulated within the cranial cavity.

*Xth Nerve. Vagus.*—In stimulating the uncut nerve outside the skull, below the level of its junction with the hypoglossal, rhythmical movements of swallowing were produced, which occurred at the rate of twenty-five times in thirty-five seconds.

In one observation all the constrictors of the pharynx were thrown into action, when the peripheral end of the cut nerve was stimulated outside the skull.

The rhythmical movements of swallowing obtained by stimulating this nerve must be due to, of course, the simple reflex, the stimulus acting on the nerve in the centripetal direction, and that this was the case is proved by the fact that no movement was obtained when the peripheral end of the cut nerve was stimulated inside the skull.

* Without definitely supporting this view, Gaskell ('Roy. Soc. Proc.,' vol. 43, p. 390) shows that some large "somatic" nerve-fibres leave the facial nerve between its origin from the bulb and its exit from the stylomastoid foramen. He suggests that some of them may possibly form a nerve to supply the levator palati, but he leaves their real destination undetermined.

The superior laryngeal branch on being stimulated gave rhythmical movements of swallowing at the rate of seventeen times in fifteen seconds, but when the nerve was cut and its peripheral end stimulated, only very slight movement was produced in the larynx, evidently by contraction of the cricothyroid muscle.

*XIth Nerve. Accessory to Vagus.*—In discussing the motor functions of the VIIth nerve, we stated that the hitherto received idea of the soft palate being supplied by the facial nerve was, according to our observations, entirely erroneous. We find that the levator palati is supplied entirely by the XIth nerve.* When the peripheral end of the cut nerve was stimulated inside the skull, elevation of the soft palate on the same side was invariably seen. The path by which the fibres from this nerve reach the palate is probably through the upper branch of the pharyngeal plexus.

*XIIth Nerve. Hypoglossal.*—When the entire nerve was excited outside the skull, just below the point where it is joined by the first cervical nerve, the tongue was flattened posteriorly on the same side, and the tip protruded also on the same side, while in no case was there any heaping up of the tongue.

At the same time the depressors of the hyoid bone were thrown into action, and in some cases this dragging downwards of the hyoid completely prevented the tongue from being protruded.

The movements described above were repeated without alteration when the peripheral end of the cut nerve was excited at the same place.

It must be particularly noted that the movements of the tongue were purely uni-lateral, and this was proved to be the case beyond doubt by two experiments, in which the tongue was divided longitudinally in the middle line to the hyoid bone when the movements were seen to be entirely confined to the side stimulated.

When the cut nerve was excited within the skull a different result was obtained, the tongue was flattened behind, and protruded towards the same side, but there was no action in the depressors of the hyoid.

It has always been held that the depressors of the hyoid bone receive their motor nerve supply from the hypoglossal through the descendens noni, but, as will be shown further on, according to our observation, these muscles are supplied by the first and second cervical nerves, and it is only when the hypoglossal is stimulated below the point where it is joined by the branch from the first cervical nerve, that any movement is produced in the depressors of the hyoid.

* I desire to add here that Dr. Felix Semon, in the course of some experiments *(unpublished)*, performed in conjunction with myself, found that in the dog the *levator palati* was innervated by the XIth nerve.—V. H.

B. *Spinal Division.*

Our observations of the motor functions of the first three cervical nerves as regards their influence on the hyoidean muscles have been made when the nerves have been excited—

(*a.*) In the spinal canal.

(*b.*) In the neck immediately upon their exit from between the vertebral transverse processes.

The nerves in the spinal canal were separated from the spinal cord and thoroughly dried, the efficacy of the precautions taken against spread being evidenced by the difference in result obtained by exciting each root.

The effects obtained by the methods *a* and *b* were identical.

*Ist Cervical Nerve. Branch of Union with the Hypoglossal.*—In the description of the XIIth cranial nerve, we have stated as the result of our experiments that the depressors of the hyoid bone are not thrown into action when this nerve is stimulated within the skull. On carefully dissecting out the branch from the Ist cervical nerve to the hypoglossal we find on excitation of it that there is no movement in the tongue, but the depressors of the hyoid bone are strongly contracted. Of these muscles the sterno-hyoid and sterno-thyroid were always especially affected, while the omo-hyoid was less frequently seen to contract and in some cases not at all. In the cases where this muscle contracted, in one experiment the anterior belly alone acted, and when both bellies contracted the movement in the anterior was in excess of the posterior.

*IInd Cervical. Branch to the Descendens Noni.*—On stimulating this nerve the depressors of the hyoid were thrown into action, but the muscles involved were not affected in the same way as was the case with the Ist cervical nerve. The muscle which was most constantly set in action by excitation of the IInd cervical nerve was the omo-hyoid and especially its posterior belly. The sterno-hyoid and sterno-thyroid also took part in depressing the hyoid bone, but it was especially remarked in half the cases, that their action was notably less powerful than that of the omo-hyoid. In one experiment in which a very weak current was employed, the omo-hyoid was alone seen to contract. We are consequently led to conclude that while the sterno-hyoid, sterno-thyroid, and omo-hyoid muscles are all set in action by excitation of the Ist and IInd cervical nerves, the first two muscles are relatively supplied by the former nerves, while the IInd nerve is especially connected with the omo-hyoid muscle.

*Descendens Noni.*—We prefer to mention here the results of exciting this nerve, inasmuch as we regard its motor fibres to be derived entirely from the Ist and IInd cervical nerves. This nerve

(ordinarily regarded as a branch of the XIIth cranial), when stimulated above its junction with the branch from the IInd cervical nerve, produced contraction of the sterno-hyoid and sterno-thyroid muscles, and where the current employed was weak there was no contraction of the omo-hyoid, but this movement was superadded on increasing the strength of the current.

We ought here to mention the opinion held by Volkmann (*loc. cit.*, *infra*) that fibres ascend to the hypoglossal from the spinal rami communicantes by the descendens noni.

*IIIrd Cervical Nerve.*—On stimulating the branch from this nerve, which forms the IInd cervical nerve just before the ansa thus formed is connected to the descendens noni, there was no action seen in the depressors of the hyoid bone; it therefore seems certain that these muscles are supplied with motor fibres solely by the branches from the Ist and IInd cervical nerves.

Summary of Results.

| Cranial nerves. | Reference. | Views previously held. | Authors' views. |
|---|---|---|---|
| V. Trigeminus. | All authors. | Muscles of mastication. | Ditto. |
| VII. Facial. | Hermann, 'Physiology.' | Muscles of face, stylo-hyoid, levator palati, digastric (posterior belly), stapedius, platysma myoides. | In agreement, except with regard to the levator palati, for which see XIth nerve. |
| | Quain's 'Anatomy,' 9th edition. | Muscles of face and of tympanum, the levator palati, azygos uvulæ (through the large superficial petrosal nerve). | |
| | Ellis' 'Anatomy,' 10th edition. | "Supposed" to send motor fibres to Meckel's ganglion and so to palate. | |
| | Bastian, 'Cerebral and Bulbar Paralysis.' | Expresses great doubt as to the superficial petrosal nerve supplying the soft palate. | |
| | Hughlings Jackson. | *Vide* XIth nerve. | |
| | Volkmann, 'Müller's Archiv,' 1840, p. 475. | No movement of soft palate. | |
| | Hein, 'Müller's Archiv,' 1844, p. 297. | No movement of soft palate. | |

Summary of Results—*continued.*

| ...l nerves. | Reference. | Views previously held. | Authors' views. |
|---|---|---|---|
| ...tympani. | Hermann, *loc. cit.* | Secretory and (?) gustatory functions. | In agreement with secretory functions; certainly *not* motor. |
| | Quain, *loc. cit.* | Submaxillary gland and "tongue." | |
| | Bastian, *loc. cit.* | Secretory and gustatory functions. | |
| ...ossopharyngeal. | Hermann, *loc. cit.* | Levator palati, azygos uvulæ, middle constrictor of pharynx, stylopharyngeus. | Stylopharyngeus, (?) middle constrictor of pharynx. |
| | Quain, *loc. cit.* | Stylopharyngeus. | |
| | Bastian, *loc. cit.* | Middle constrictor of pharynx, stylopharyngeus, azygos uvulæ, levator palati. | |
| | Volkmann, *loc. cit.* | Middle constrictor of pharynx, stylopharyngeus. | |
| ...gus. ...ccessory. } | Hermann, *loc. cit.* | Muscles of soft palate and pharynx, larynx, and alimentary canal. | X. Vagus, nil motor in neck and head. |
| | Quain, *loc. cit.* | "Combined X and XI" form the pharyngeal plexus . . . which supplies the muscles and mucous membrane of larynx. | |
| | Hein, *loc. cit.* | Movements of soft palate. | |
| | Bastian, *loc. cit.* | The larynx, pharynx, &c. | |
| ...gus (only). | Volkmann, *loc. cit.* | Levator palati, azygos uvulæ (goat), constrictors of pharynx (superior and inferior), palato-pharyngeus, laryngeal muscles. | |
| | Chauveau, quoted by Vulpian. | Do. (donkey and horse). | |
| | Vulpian, 'Comptes Rendus,' vol. 103, 1886. | Do. (dog). | |
| ...ccessory to ...agus. | Bastian, *loc. cit.* | In all probability it supplies the levator palati. | Excitation of it produces elevation of soft palate on same side, in addition |

Summary of Results—*continued.*

| Cranial nerves. | Reference. | Views previously held. | Authors' vi |
|---|---|---|---|
| | | | to moveme the pha larynx, &c stated by authors. |
| | Hughlings Jackson, 'London Hospital Rep.,' vol. 1, p. 335, 1864. | Denies that motor fibres of soft palate come from facial nerve, and supports the belief that they come from vagus, or its accessory nerve. | |
| XII. Hypoglossal. | Hermann, *loc. cit.* | Muscles of tongue, muscles connected with hyoid bone, and it receives sensory fibres through its ramus descendens from the first cervical nerves. | Intrinsic mu of tongue c same side *Not* the de sors of l bone. |
| | Quain, *loc. cit.* | It supplies, alone or in union with spinal nerves, the tongue, muscles, and depressors of the hyoid bone. | |
| | Volkmann, *loc. cit.* | As a rule no movement in depressors of hyoid, but the sterno-hyoid was seen to contract on intracranial excitation in two calves and one dog. | |
| | Bastian, *loc. cit.* | Motor to tongue and most of the muscles attached to hyoid bone. | |

| 'ervical nerves. | Reference. | Views previously held. | Authors' views. |
|---|---|---|---|
| I. | All authors.<br>Volkmann, *loc. cit.* | Nil save posterior neck muscles.<br>Sterno-hyoid and sterno-thyroid supplied. | I. Depressors of hyoid, especially sterno-hyoid and sterno-thyroid. |
| II.<br>III. | Hermann, *loc. cit.*<br>Quain, *loc. cit.*<br>Bastian, *loc. cit.*<br>Other authors. | "Supply" infra-hyoid muscles with descendens noni. | II. Depressors of hyoid, especially omo-hyoid.<br>III. Nil motor. |
| II (alone). | Volkmann, *loc. cit.* | Supplies the small muscles of neck, but not the depressors of hyoid. | |

"An Additional Contribution to the Placentation of the Lemurs." By Professor Sir WM. TURNER, Knt., M.B., LL.D., F.R.S. Received May 21, 1888.

n 1876 I contributed to the Royal Society a memoir "On the centation of the Lemurs," which was published in the 'Philosocal Transactions' of that year (vol. 166, Part 2). The gravid uteri ch I examined and described were from specimens of *Propithecus lema*, *Lemur rufipes*, and *Indris brevicaudatus*. The examination wed in these Lemurs that the placental villi were diffused over the ιter part of the surface of the chorion, so as to approximate in eral plan with the arrangement in the Pig, Mare, and Cetacean, ıgh of course with special characters of their own; that there e also distinct areas on the chorion free from villi; that the ·ine mucous membrane possessed multitudes of crypts, from out vhich the villi were easily drawn; that smooth patches of mucous ıbrane devoid of crypts, and corresponding to the non-villous s of the chorion were present, towards which the stems of the ine glands converged in a remarkable manner, and on the surface vhich they opened by obliquely directed mouths in considerable ıbers. Further, it was pointed out that the chorion occupied both ıs of the uterus, though the part which was prolonged into the ·gravid horn was *only a short* diverticulum, and that the allantois 'ed *a large persistent sac*, which, like the sac of the amnion, did

not extend into the diverticulum of the chorion. The specimens were at different stages of gestation, but none was at the full time, though the fœtus of *Propithecus* was well developed, and measured, without including the tail, 5 inches in length.

In April of the present year I received from F. E. Beddard, Esq., Prosector to the Zoological Society of London, the gravid uterus of a Lemur, which he informs me was *Lemur xanthomystax*. The animal had died during labour. On examining the specimen the uterus showed no signs of inflammation, but its posterior wall was ruptured immediately above the line of reflection of the peritoneum from the rectum on to it. The caudal end of the fœtus occupied the upper third of the vagina, the membranes having been torn so as to allow the passage of the hinder part of the trunk out of the uterus. The uterine vessels were then filled with a carmine and gelatine injection, and the vessels of the chorion were partially filled with a blue injection through the umbilical trunks.

The uterus was somewhat smaller than that of *Propithecus diadema*, described in the memoir above referred to. As in that specimen it seemed on external examination as if it were a single uterus, but when opened into it was seen to possess a largely dilated left cornu, containing the head of the fœtus, and a short right cornu, dilated to about the size of a walnut, both of which freely communicated with the cavity of the corpus uteri; a depending fold of mucous membrane not half an inch deep separated the cornua from each other. The vagina was about 60 mm. long, and with a smooth mucous membrane. The os uteri was defined by a circular fold of mucous membrane. Each ovary was only about half the size of a common pea, and the left one contained a highly vascular corpus luteum.

The folds and sulci of the mucous membrane both of the corpus and cornua uteri with their numerous crypts, corresponded generally with those previously described and figured by me in *P. diadema*. The largest area of smooth mucous membrane was immediately above the os uteri; that next in size was situated around the orifice of the left Fallopian tube, whilst a smaller one surrounded the opening of the right tuba. Smooth areas were interspersed amidst the mucous folds; they were much less vascular than the folds and crypts, but as, both in their appearance to the naked eye and their relation to the openings of the uterine glands, they corresponded closely to what I have previously described in *P. diadema* and *Lemur rufipes*, I need not further describe them. The epithelial contents gave to the uterine glands a yellowish colour; but it was difficult to individualise in them the separate cells, the contents of which were granular, and the outlines indistinct. It seemed indeed as if the cells were in process of degeneration, owing to the period of gestation having come to an end, and as parturition had begun, the glands were no longer required to

take a part in the nutrition of the fœtus. The gland-layer of the mucous membrane was readily distinguished subjacent to the crypt-layer.

The folds and crypts surrounding the smooth areas of the mucosa were highly vascular. The crypts opened freely on the surface, and to some extent smaller secondary crypts branched off from the larger depressions. The distribution of the compact capillary network in the walls of the crypts resembled the arrangement previously figured in *P. diadema.*

I drew the chorion away from the uterine mucosa by gentle traction, and in the process of detachment the villi came out of the crypts with great ease. A considerable area of chorion next the os uteri, some of which had been torn in the descent of the fœtus, was free from villi and not very vascular. As one traced the chorion from the os, short scattered villi in the first instance projected from it, to be succeeded still further away by longer and broader villi arranged either in tufts or rows, the size and arrangement of the villi being adapted to the crypts in the mucosa. Opposite the uterine opening of the left Fallopian tube an area of the chorion about 33 mm. in its longest diameter was smooth and free from villi: it was placed at the end of the chorion furthest removed from the os uteri. A much smaller non-villous area of chorion corresponded to the opening of the right tuba, and was much nearer to the os than was the case with the non-villous area opposite the left tuba; in the right cornu the villi were arranged in low ridges, and the ridges and furrows in the uterine mucous membrane were shallow. Owing to the shortness of the right uterine cornu, the chorion lodged within it formed only a slight projection of the general bag of the chorion. Smooth patches of chorion, in apposition with the corresponding smooth areas of the mucosa, were interspersed amidst the rows and tufts of villi which covered so large a proportion of the free surface of the chorion.

The blue injection which had been passed into the umbilical trunks had filled the vessels ramifying in the deeper layer of the chorion, which could be seen both in the villous and non-villous parts of the membrane not unfrequently having a tortuous course. Opposite the bases of the villi these vessels gave off small branches which entered the villi and formed in them a close network of capillaries.

The large sacs both of the amnion and allantois in *L. xanthomystax* closely corresponded in arrangement with those previously described by me in *Lemur rufipes.*

The fœtus was 19 cm. long from the tip of the nose to the root of the tail, and the tail was 14 cm. long. It was evidently quite mature and the hairs and nails were well developed. The lower incisors had partially cut the gum. Both in this specimen and in the Propithecus *diadema previously described* the breech was the presenting part, and

the head was near the Fallopian tube belonging to the more dilated of the two uterine cornua. In three specimens of *Lemur rufipes* described in my previous memoir, the head was in proximity to the os, and the caudal end of the fœtus was in the more dilated horn. It would appear, therefore, that in the Lemurs, either the head or breech may be the part of the animal first to be born.

The examination of the gravid uterus of *Lemur xanthomystax* confirms, therefore, the conclusions to which both Alphonse Milne Edwards* and I had arrived independently in our previous investigations, that the placenta in this important group of animals is diffused and non-deciduate, and that the sac of the allantois is large and persistent up to the time of parturition. In these important respects, therefore, the Lemurs are, in their placental characters, as far removed from man and apes as it is possible for them to be.

Although I am not disposed to attach too much weight to the placenta as furnishing a dominant character for purposes of classification, yet I cannot but think that animals which are megallantoid, non-deciduate, and with the villi diffused generally over the surface of the chorion, ought no longer to be associated in the same order with animals in which, as in the apes, the sac of the allantois early disappears, and the villi are concentrated into a special placental area, in which the fœtal and maternal structures are so intermingled that the placenta is highly deciduate. Hence I am of opinion that the Lemurs ought to be grouped apart from the Apes in a special order, which may be named either with Alphonse Milne Edwards *Lemuria*, or with Victor Carus and others *Prosimii.*

## Addendum.—June 2.

After the fœtus had been mounted for preservation in spirit, delicate flakes of a translucent cuticular-looking membrane were seen partially to float off from the surface of the abdomen and from the ventral surface of the limbs. In the groins and axillæ the membrane was very distinct, and formed an almost complete covering for the surface of the limbs external to the hairs, which, though of some length, were few in number, and scattered over the surface of the skin. On the dorsal aspect of the fœtus, both on the head, trunk, and limbs, where the hairs were longer and closely set together, the flakes were much more fragmentary and over considerable areas were absent. The appearance presented was such as to lead to the impression that flakes of a cuticular membrane, subjacent to which the hairs had been developed, were in process of being shed.

* "*Histoire* Naturelle des Mammifères de Madagascar," forming vol. 6, chap. ix, of *Grandidier's* 'Histoire de Madagascar.'

A number of years ago, Professor Hermann Welcker, of Halle, described by the name of *Epitrichium* a cuticular membrane, situated superficial to the hairs, which formed a complete envelope to the fœtus of *Bradypus tridactylus*, *Cholopus didactylus*, *Myrmecophaga didactyla*, and *Dicotyles*. He figured it *in loco* both in *Bradypus* and *Dicotyles*.* It was obviously quite distinct from the amnion.

In a memoir "On the Placentation of the Sloths," published in 1873, I described and figured the epitrichium in *Cholopus hoffmanni*,† and stated that I had also seen a similar arrangement in a fœtus of *Bradypus tridactylus*. In a subsequent dissection of the gravid uterus of *Bradypus tridactylus* I have recognised that this membrane in its relations to the fœtus corresponded with Welcker's figure and description. In these animals the epitrichium formed a complete covering of the fœtus, and closely followed the contour of the head, trunk, and limbs, immediately external to the hairy coat which was situated in the interval between the epitrichium and the skin; though the epitrichium was perforated at the muzzle by the long tactile hairs which grew from the lips. It was adherent to the cuticle of the margins of the eyelids, of the orifice of the nose, mouth, external auditory meatus, and anus, and was also attached to the soft cuticle around the roots of the claws. It was entirely distinct from the amnion, and from its relations to the hairy coat was obviously the layer of the epidermis situated superficial to the hairs, and which had become elevated as a distinct and continuous membrane as a result of their development and growth.

From its relation to the hairy coat, the cuticular membrane on the fœtus of *Lemur xanthomystax* was without doubt a similar structure to the epitrichium investing the fœtus of the Sloths, but with this difference, that instead of forming a continuous envelope around the head, body, and limbs of the fœtus, it was broken up into flakes or patches, which were the best marked where the hairs were scattered, and had almost disappeared in the mature fœtus, where the hairy coat was thick and abundant.

The recognition of this membrane in *Lemur xanthomystax* led me to examine the fœtus of *Propithecus diadema*, referred to in my memoir "On the Placentation of the Lemurs," with the view of seeing if a corresponding structure was present. I found on immersing the fœtus in water, or in spirit, that similar membranous flakes floated off from the surface of the hair. In some localities they were so loose as to make it difficult to say what their original relation to the hairs had been, but in other places the membrane had not been disturbed, and the hairs were situated between it and the

* "Ueber die Entwicklung und den Bau der Haut und der Haare bei Bradypus," in 'Abhandl. der Naturforsch. Gesellschaft zu Halle,' vol. 9, 1864.

† '*Edinburgh Roy. Soc. Trans.*,' vol. 27.

surface of the skin. It must be understood that this membrane was quite distinct from the amnion.

The epitrichium, therefore, is present both in the Lemurs and in the Sloths, but in the former it does not, after the hairy coat is developed, form a complete envelope for the fœtus, but is broken up before the termination of the period of gestation into more or less detached flakes of membrane.

## III. "Note on the Coagulation of the Blood." By L. C. WOOLDRIDGE, M.D., M.R.C.P., Co-Lecturer on Physiology at Guy's Hospital. Communicated by Professor VICTOR HORSLEY, F.R.S., &c. (From the Laboratory of the Brown Institution.) Received May 24, 1888.

In a paper read before the Royal Society, April 26th, 1888, Dr. Halliburton offers some criticism of my views respecting the coagulation of the blood. In this note I shall briefly summarise and traverse the objections Dr. Halliburton raises to my theory and experiments.

I. Dr. Halliburton suggests that the substance I call A-fibrinogen —which I obtained by cooling peptone-plasma—is not a normal constituent of the blood plasma, but that it is a precipitate of a hemi-albumose, supposed by him to be present in the peptone which is injected into an animal for the purpose of obtaining peptone plasma. I do not use Witte's peptone, as Dr. Halliburton appears to have done, on account of its recognised impurity, but that obtained from Dr. Gruebler's well-known laboratory in Leipsic. This peptone is prepared according to Henniger's method. A 10 per cent. solution of it in ½ per cent. solution of sodium chloride is quite clear after filtration.

It gives no precipitate on cooling to zero.

It disappears wholly from the blood within one or two minutes after injection.

Finally, A-fibrinogen has properties absolutely different from the peptone injected.

Dr. Halliburton appears to think that this substance, A-fibrinogen, exists only in peptone plasma.

I stated in a paper read before the Royal Society in 1885, "On a New Constituent," &c., that it was also present in salt plasma, and I gave details concerning it in the Croonian MS., which is in the archives of the Royal Society. I explained at length in the paper referred to by Dr. Halliburton, and published in Ludwig's 'Festschrift,' 1887, *why there are*, as has long been known, two varieties of salt plasma, *namely, one* containing, as I showed, no A-fibrinogen, this being not

spontaneously coagulable, the other containing it, and therefore being spontaneously coagulable.

II. Dr. Halliburton further asserts, that whereas in the abstract of the Croonian Lecture, I described a body, B-fibrinogen, in the paper in Ludwig's 'Festschrift,' published shortly afterwards, this body was not mentioned, or had become identical with the fibrinogen of Hammarsten. This statement is totally incorrect, for on page 228 of Ludwig's 'Festschrift' there will be found a paragraph headed "B-fibrinogen," and on the following page this passage occurs: "Man sieht also dass das Fibrinogen von Hammarsten in Plasma einen Vorgänger hat, welche andere Eigenschaften besitzt, und ich bezeichne diese Substanz als 'B-fibrinogen.'" The differences between the two bodies here referred to are precisely those mentioned in the abstract of the Croonian lecture, and are shortly as follows:—

(*a.*) B-fibrinogen does not clot with fibrin ferment, but it does clot with leucocytes and other animal and vegetable cells.

(*b.*) It clots with substances which can be obtained from these animal and vegetable cells in large quantities, by extraction with water. These substances I call tissue fibrinogens.

(*c.*) It further clots with lecithin.

Hammarsten's fibrinogen, in remarkable contrast with the properties of this body, does not clot with leucocytes or other animal or vegetable cells, nor does it clot with the substances called tissue fibrinogens nor with lecithin.

I would here add that the fibrinogen in most transudation fluids is similar to Hammarsten's fibrinogen. I have clearly indicated these differences in previous publications.

III. With regard to Dr. Halliburton's remark on the relation of lecithin to clotting, I may say that it not only gives rise to clotting in peptone plasma and cooled plasma, but in a solution of fibrinogen isolated from salt plasma and in the plasma obtained from the blood after the injection of tissue fibrinogen. In discussing the experiments on the behaviour of cooled blood towards lecithin, Dr. Halliburton does not recount the details of the experiments, and hence he conveys a misleading impression of the same. It is necessary for these experiments to use a finely particulate and yet thick emulsion of lecithin, for the following very obvious reasons. The lecithin is insoluble in the salt solution into which the blood is received, and a large quantity of blood being received into a relatively small quantity of the salt solution, the lecithin does not come into contact with all the plasma unless a fine thick emulsion be used.

The fact that fluids free from lecithin produce clotting, in no way disproves the contention that lecithin is an essential factor in coagulation, since every variety of fibrinogen contains lecithin. Lecithin

is, next to proteid, the most widely distributed substance in the animal organism. As Hammarsten has well said, "it has been found wherever it has been looked for." Whenever I have stated that lecithin is present in any fibrinogen, I have prepared it and tested for it in the way I have previously repeatedly described in the papers Dr. Halliburton quotes.

IV. The criticisms which Dr. Halliburton passes upon my discovery that tissue fibrinogens cause intravascular clotting when injected into the living circulation, can hardly be regarded seriously; for he asserts that the tissue fibrinogen is a slimy mass, and causes clotting by mechanically plugging the vessels, whereas if he had repeated my experiments he would have found (1) that the fibrinogen is not at all slimy, and (2) that it can hardly be supposed to cause clotting mechanically, since it passes through the right heart, then the capillaries of the lungs, next the left heart and aorta, and finally the capillaries of the alimentary canal before it first causes clotting, *i.e.*, in the portal vein in the dog.

## IV. "Note on the Volumetric Determination of Uric Acid." By A. M. Gossage, B.A., Oxon. Communicated by Professor J. Burdon Sanderson, F.R.S. Received May 29, 1888.

Dr. Haycraft has recently proposed a method for the volumetric determination of uric acid in urine ('Brit. Med. Journ.,' 1885, 2, p. 1100) which has great advantages over all former methods in that it is much quicker and easier to manage. The uric acid from 25 c.c. of urine is precipitated by silver nitrate after previous addition of sodium carbonate (to prevent reduction) and ammonia (to dissolve silver chloride, &c.); this precipitate is then collected, washed, and dissolved in nitric acid, and the amount of silver present in this solution ascertained by Volhard's method, *i.e.*, titration with ammonium sulphocyanate; from this the amount of uric acid can be calculated. "In order to test the accuracy of the process," he says, "I prepared several solutions of acid urate of sodium of known strength. To these I added various quantities of common salt, magnesium sulphate, and phosphate of soda in order to imitate as far as possible the urinary secretion. On estimating the uric acid in these solutions, I obtained wonderfully correct results. In all cases not much more than a milligramme was lost during the process, and may be simply accounted for by the fact that no salt of uric acid is absolutely insoluble. . . . . In order further to test its accuracy, 50 c.c. of urine were divided into two equal portions; to the first 25 c.c. of a solution of acid urate of sodium of known strength were added; to

the second 25 c.c. of water were added. When estimated the two fluids should show a difference equal to the quantity of salt added." Results very closely corresponding to this were obtained.

These results do not agree with results obtained by Salkowski (Pflüger's 'Archiv,' vol. 5, 1872, p. 210) and Maly (Pflüger's 'Archiv,' vol. 6, p. 201). Salkowski proposed a volumetric method for the determination of uric acid, very similar to that proposed by Dr. Haycraft; in this he added excess of silver nitrate and estimated the excess of silver present. He gave up this method, however, as on examining the silver precipitate obtained from urine, after complete precipitation of the phosphates by magnesia mixture, he found that it contained magnesium as well as silver, and that the proportion of magnesium to silver varied considerably in precipitates from different urines, though constant for the same urine. Haycraft considers that the presence and variation in amount of magnesium in these precipitates may be due to varying quantities of magnesium ammonium phosphate in them. This is, however, impossible, as the phosphates were precipitated by Salkowski previously, and the urine allowed to stand for twenty-four hours before filtration to ensure their complete separation. Salkowski's results were confirmed by Maly, who found that if in the presence of salts of calcium, magnesium, potassium, and ammonium, a solution of a urate be precipitated by silver nitrate, the precipitate contains these metals as urates as well as silver urate.

As a test of the accuracy of Haycraft's method, I examined samples of various urines both by his method and by Salkowski's method, which is universally acknowledged to be the most reliable, and the accuracy of which has been proved by experimental evidence. This method consists in taking 250 c.c. of urine, adding 50 c.c. of magnesia mixture to precipitate phosphates, and then adding to 240 c.c. of the filtrate (which are equivalent to 200 c.c. of the urine) silver nitrate to precipitate the uric acid. This precipitate of silver urate is decomposed by sulphuretted hydrogen after being suspended in water. The liquid is then acidified, filtered hot, and evaporated to small bulk, and the uric acid allowed to crystallise out. These crystals are then dried and weighed. The following results were obtained:—

1 c.c. $NH_4CNS$ = 0·00168 Uric Acid.

| Expt. | Salkowski (200 c.c. urine). | | Haycraft (25 c.c. urine). | | |
|---|---|---|---|---|---|
| | Quantity of uric acid obtained. | Mean percentage. | No. of c.c. of $NH_4CNS$ required. | Mean equivalent quantity of uric acid. | Percentage. |
| I. | 0·168 gr. | 0·084 | (a.) 16·2 c.c.<br>(b.) 16·3 „ | 0·027 | 0·108 |
| II. | 0·07 gr. | 0·035 | (a.) 11·8 c.c.<br>(b.) 11·4 „<br>(c.) 11·0 „ | 0·019 | 0·076 |
| III. | (a.) 0·098 gr.<br>(b.) 0·1045 „ | 0·051 | (a.) 12·3 c.c.<br>(b.) 12·1 „ | 0·0205 | 0·082 |
| IV. | (a.) 0·068 gr.<br>(b.) 0·073 „ | 0·035 | (a.) 10·7 c.c.<br>(b.) 11·1 „ | 0·018 | 0·072 |
| V. | (a.) 0·154 gr.<br>(b.) 0·160 „<br>(c.) 0·165 „ | 0·08 | (a.) 16·3 c.c.<br>(b.) 16·4 „ | 0·027 | 0·108 |

The results obtained by Haycraft's method were always considerably higher than those obtained by Salkowski's. The reason of this is that Dr. Haycraft has assumed that the silver precipitate from urine consists of an urate containing only 1 atom of silver in the molecule, whereas the proportion of silver in this precipitate is always larger, and varies in amount in different urines. If we assume that the precipitate contains 2 atoms of silver in a molecule of urate and divide the results obtained by Haycraft's method by two, we see that in two cases they are about equal to, in the rest less than those obtained by Salkowski's method. The proportion of the results obtained by one method to those obtained by the other varies. This agrees with the results of Salkowski's researches, from which one would expect that the results obtained by Haycraft's method would not bear a constant relation to the results obtained by Salkowski's, and that the halves of the results by the former method would be lower than, in most cases, those obtained by the latter.

"On the Effects of Increased Arterial Pressure on the Mammalian Heart." By JOHN A. MCWILLIAM, M.D., Professor of the Institutes of Medicine in the University of Aberdeen. Communicated by Professor M. FOSTER, Sec. R.S. Received May 30, 1888.

'he following is a short preliminary statement of some of the main ts elicited in the course of a recent investigation. The experiments :e conducted on chloroformed cats. The thorax was laid open, ificial respiration being maintained, and the action of the auricles l ventricles was recorded by means of the graphic method. The ıtraction of the heart in ordinary circumstances having been ierved and registered, the arterial pressure was raised by constrict- ; or clamping the last part of the thoracic aorta—usually for a :iod of 4—8 seconds. Clamping for longer periods was often ›ompanied by convulsive movements of the animal.

The results may be briefly summarised as follows :—

They fall into one or other of two categories according as to whether : medullary cardio-inhibitory mechanism is (I) functionally active controlling the heart's action, or is (II) incapable of affecting the 'diac beat. The latter condition is one that may result from 'ious causes, such as (*a*) section of the vagus nerves or paralysis of ir function through the influence of drugs, &c.; (*b*) depression or 'alysis of the medullary cardio-inhibitory *centre*, brought about by gs or by other causes.

. In the first-mentioned condition, when the cardio-inhibitory :hanism is in a position to control the heart's action, a marked ı of the arterial pressure (such as results from compression of the cending aorta) causes, as Marey has shown, a slowing of the cardiac thm.

find that the rise of blood-pressure also causes marked changes in contraction force of the cardiac muscle. For a short time (a few ›nds, 1, 2, 3, &c.), after clamping of the descending aorta there urs an augmentation in the strength of the beat—especially of the tricular beat; meanwhile the rhythm has become slower than ›re (fig. 1).

'hen there occurs a more or less sudden change. The auricular tractions undergo a striking diminution in force. They remain eebled until the compression of the aorta has been discontinued ı the blood pressure has fallen; then they gradually recover, ugh the process of recovery may not always begin at once (figs. 1 l 2).

'*he changes in the vertricular* action consequent upon closure of


Fig. 1


Fig. 2

the descending aorta do not run parallel with those occurring in the auricles. The ventricles, while they beat more slowly than before, usually beat much *more strongly* even when the auricular contraction has become markedly weakened (fig. 1). Depression of the ventricular force may occur, but it comes considerably later than the auricular depression, and is very much slighter in degree (fig. 3). The slow strong ventricular systoles are able to empty the cavity of the left ventricle when systoles of less strength fail to do so—as indicated by the fact that the recording lever often fails to descend to the ordinary level in the interval between the contractions (fig. 1). When the descending aorta has been released and the pressure has fallen, a period of marked cardiac acceleration often succeeds; during this acceleration, the individual ventricular beats are much diminished in force (fig. 3).

The above-mentioned cardiac changes attendant on a sudden rise of arterial pressure are brought about through the medullary cardio-inhibitory centre and the vagus nerves. They are of such a nature that while the ventricles are contracting slowly and powerfully in such a way as to be able to discharge their contents in spite of the increased arterial pressure, there occurs a striking change in the action of the auricles involving a great reduction in the amount of blood pumped into the ventricles and the degree in which the latter are distended just before their systole. Hence the quantity of blood thrown out by the left ventricle into the systemic arteries is much diminished, and the rise in the blood-pressure is in some measure counteracted and controlled.

II. In conditions where the medullary cardio-inhibitory mechanism has ceased to exert any controlling influence upon the heart (*e.g.*, after section of both vagi), the effects following a sudden rise of arterial pressure are entirely different from those above described.

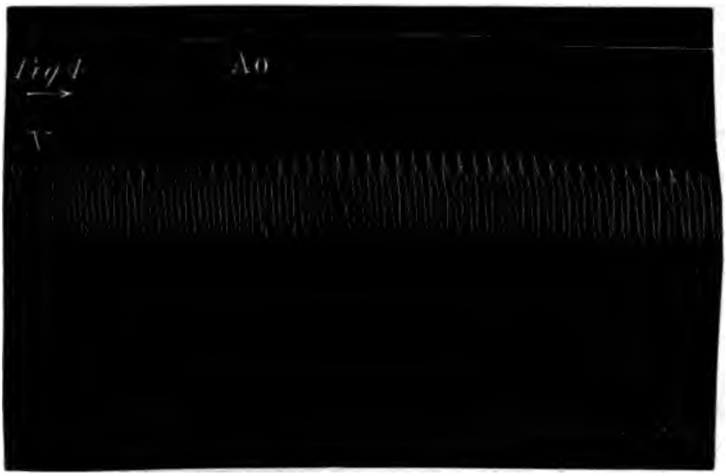
Marey showed that there was no very constant relation between the rate of the heart's action and the height of the blood-pressure after section of the vagi; some degree of acceleration was commonly observed.

Examining the cardiac changes in the way already mentioned, I find that after section of the vagi or paralysis of the medullary cardio-inhibitory mechanism, a sudden rise of arterial pressure causes no very striking or constant change in the heart's rhythm; frequently there is *slight* acceleration. There is a complete absence of the characteristic changes in the contraction force above described (under I). As regards the strength and character of the cardiac action, there are two conditions to be noted.

(1.) The heart may at each systole be able to discharge its contents in normal *or approximately normal* fashion. In such circumstances *the principal change to be observed* in a vigorous heart is a marked


Fig. 5

increase in the force of the beat, at least in the ventricles (fig. 4).




(2.) On the other hand the relation of the ventricular power to the arterial resistance may be such that the left ventricle is not able to expel its contents at each beat in the normal fashion. The recording lever fails to descend to the usual level between the contractions; it remains elevated to a considerable extent from the ordinary base line.

The results occurring in both the conditions referred to—(1) and

(2)—are not obviated by section of all the visible branches of the annulus of Vieussens, and of the vago-sympathetic in the neck and thorax. They appear to depend on properties of the heart itself, and not on the influence of extra-cardiac nerves.

## DESCRIPTION OF FIGURES.

FIG. 1.—Tracing of auricles and ventricles, showing effects of clamping descending aorta (Ao.). In the ventricular tracing the upward movement indicates contraction; in the auricular tracing the downward movement indicates contraction. The time tracing shows half seconds.

FIG. 2.—Tracing of auricles. Downward movement indicates contraction. Descending aorta clamped at the point marked ↓, and released at ↑. Time marker indicates half seconds.

FIG. 3.—Tracing of auricles and ventricles. In the ventricular tracing contraction is represented by the upward movement, in the auricular tracing by the downward movement. Time marker shows half seconds. Clamping of descending aorta.

FIG. 4.—Tracing of ventricles; upward movement indicates contraction. Increase in size of beats during the closure of the descending aorta. Time marker indicates half seconds.

FIG. 5.—Tracings of auricles, ventricles and blood-pressure in left carotid artery. The lowest tracing marks the time in half seconds. The level of the ventricular tracing rises during closure of the descending aorta; there is incomplete emptying of the left ventricle at each systole.

## *Presents, June* 7, 1888.

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*June* 14, 1888.

The Right Hon. the EARL OF ROSSE, Vice-President, in the Chair.

The Right Hon. John Hay Athol Macdonald (Lord Advocate), Mr. Thomas Andrews, Mr. James Thomson Bottomley, Mr. Charles Vernon Boys, Professor Arthur Herbert Church, Professor Charles Lapworth, Professor William Ramsay, Mr. Thomas Pridgin Teale, Mr. William Topley, Professor Henry Marshall Ward, and Mr. William Henry White were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

## I. "The Minimum-point of Change of Potential of a Voltaic Couple." By G. GORE, F.R.S. Received May 26, 1888.

In a previous communication on "The Effect of Chlorine on the Electromotive Force of a Voltaic Couple" ('Roy. Soc. Proc.,' May 3rd, 1888), I described a phenomenon which I now venture to term the "Minimum-point of Change of Potential of a Voltaic Couple." In that description a "thermo-electric pile" is mentioned as having been used for the purpose of balancing the electromotive force of the couple, whilst finding the "minimum-point of change." As very few persons possess a thermo-electric pile suitable for the purpose, I have devised and employed the following arrangement by means of which the use of the pile may be dispensed with.

Take a voltaic couple, composed of an unamalgamated strip or stout wire of zinc or magnesium (the latter is usually the best), and a small sheet of platinum, immersed in distilled water; balance its electric potential through an ordinary galvanometer by that of a precisely similar couple composed of portions of the same specimens of the same metals, immersed the same moment as the other pair in a separate quantity of the same water, and gradually add to one of the two cells sufficiently small and known quantities of an adequately weak solution of known strength in a portion of the same water, of the substance to be used, until the balance is upset, and take note of *the proportions* of the substance and of water then contained in that *cell. It is more* easy to successively dilute than to successively

rengthen the solutions, and thus arrive at the "minimum-point." he method is a little less accurate than the one in which a thermo-le is employed.

By means of this method, using a couple composed of magnesium-atinum in distilled water, I have found the following "Minimum-oints of Change of Voltaic Potential," in solutions of potassic loride, potassic chlorate, hydrochloric acid, and chlorine. I selected ese substances because they were representative ones, suitable to ield results for comparison, and because they gave extreme and termediate magnitudes of the effect. The results are compared ith those obtained with a Mg + Pt couple and the thermopile.

*Potassic Chloride*, KCl.

Solution at 18° C.; "minimum-point" lay between 1 part in 3875 nd 4650 parts of water; and by the aid of the thermopile, with he solution at 17° C., "minimum-point" between 1 in 3875 and 305.

*Potassic Chlorate*, $KClO_3$.

Solution at 19° C.; "minimum-point" between in 1 in 4650 and 5166 nd with the pile, solution at 18° C., between 1 in 4920 and 5470.

*Hydrochloric Acid*, HCl.

Solution at 17° C.; "minimum-point" between 1 in 516,666 and 64,285; and by aid of the pile, solution at 19° C., between 1 in 16,666 and 574,074.

*Chlorine*, Cl.

Solution at 18° C.; "minimum-point" between 1 in 15,656,500,000 nd 19,565,210,000; and with the pile, solution at 12·5° C., between in 17,000 millions and 17,612 millions.

These results show the great degree of delicacy of each method, and he extremely large difference of proportion of different substances equired to upset the balance. The two methods agree.

By employing a great variety of dissolved substances, I have ound that nearly every such substance has a minimum proportion elow which it has no apparent effect upon the electromotive force of MgPt or ZnPt couple in distilled water; and this proportion ppears to be a constant number, dependent only upon very simple onditions, viz., unchanging composition of the voltaic couple and iquid, a uniform temperature, and employing the same galvanometer. The apparently constant numbers thus obtained may probably be sed as tests of the purity or of the uniformity of composition of issolved substances.

*The "minimum-point"* and degree of sensitiveness varies with,

1st, the chemical composition of the liquid; 2nd, the kind of positive metal; 3rd, to a less degree with the kind of negative metal; 4th, the temperature at the surface of the positive metal, and at that of the negative one; and 5th, with the kind of galvanometer employed.

The order of the degree of sensitiveness or magnitude of the "minimum-point" is manifestly related to that of degree of chemical energy of the liquid, and, therefore, also to the atomic and molecular weights of the dissolved substances, and to the ordinary chemical groups of halogens. With certain exceptions, it is also distinctly related to the amounts of chemical heat. The greater the degree of free chemical energy of the dissolved substance, and the greater its action upon the positive metal, the smaller the proportion of it required to upset the balance. The proportion necessary for this purpose probably represents a fixed amount of voltaic energy in all cases, viz., the amount necessary to overcome the mechanical inertia of the needle of the particular galvanometer employed.

As the "minimum-point" of a chemically active substance dissolved in water is usually much altered by adding almost any soluble substance to the mixture, measurements of that point in a number of liquids at a given temperature with the same voltaic pair and galvanometer, will probably throw some light upon the state of combination and degree of chemical freedom of substances dissolved in water.

## II. "On the Change of Potential of a Voltaic Couple by Variation of Strength of its Liquid." By G. Gore, F.R.S. Received May 31, 1888.

Having found a thermo-electric pile (see 'Birmingham Phil. Soc. Proc.,' vol. 4, p. 130) convenient in detecting and measuring small changes of voltaic potential ('Roy. Soc. Proc.,' May 3rd, 1888), I have taken advantage of that circumstance to measure by the method of balance the above phenomenon in various liquids.

The following are a few examples of measurements thus made of the influence of varying quantities of different substances upon the electromotive force of a voltaic couple composed of zinc and platinum immersed in distilled water:—

Table I.—$KClO_3$ in 465 grains of Water at 16° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 39 | 1·0228 | 18 | 1·0171 |
| 36 | ” | 15 | 1·0142 |
| 33 | ” | 12 | 1·0056 |
| 30 | ” | 9 | 0·9999 |
| 27 | ” | 6 | 0·9828 |
| 24 | ” | 3 | 0·9282 |
| 21 | ” | | |

The strongest of the above solutions was a saturated one.

Table II.—Ditto at 10° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 3 | 0·9282 | 2·1 | 0·9170 |
| 2·7 | 0·9227 | 1·8 | 0·9084 |
| 2·4 | 0·9198 | Water alone. | ” |

The electromotive force gradually increased with the strength of the solution until 21 grains of the salt had been added; it then remained uniform up to the point of saturation. The total increase of electromotive force was 0·1144 volt. The smallest proportion of salt required to upset the balance of the couple was 1 part in between 221 and 258 parts of water.

Table III.—$KCl$ in 465 grains of water at 12° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 147 | 1·15436 | 57 | 1·15436 |
| 129 | ” | 39 | ” |
| 111 | ” | 21 | ” |
| 93 | ” | 3 | ” |
| 75 | ” | | |

The strongest of these solutions was saturated with the salt.

Table IV.—Ditto at 8° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·003 | 1·1546 | 0·001001 | 1·0228 |
| 0·002667 | 1·1171 | 0·000669 | 0·9942 |
| 0·002334 | 1·0543 | 0·000336 | 0·937 |
| 0·002001 | 1·0943 | 0·000224 | „ |
| 0·001668 | 1·080 | 0·000112 | „ |
| 0·001335 | 1·0514 | Water. | „ |

The electromotive force gradually increased with the strength of the solution up to 0·002 grain of the salt, then decreased, and afterwards increased again up to 0·003 grain, and then remained constant until the saturation point was attained. The total increase of electromotive force was 0·21736 volt. The minimum proportion of chloride necessary to upset the balance of potential of the couple lay between 1 part in 695,067 and 1,390,134 parts.

Table V.—HCl in 465 grains of Water at 16·5° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·15 | 1·3487 | 0·05628 | 1·1715 |
| 0·1407 | 1·2945 | 0·04691 | „ |
| 0·1313 | 1·2459 | 0·03754 | 1·1658 |
| 0·1219 | 1·2373 | 0·02816 | 1·1515 |
| 0·1125 | 1·1915 | 0·01879 | 1·1429 |
| 0·10314 | 1·1615 | 0·00942 | 1·1286 |
| 0·09377 | „ | | |
| 0·0844 | „ | 0·00005 | 1·0228 |
| 0·07502 | „ | 0·0000474 | 0·9799 |
| 0·06565 | „ | Water. | „ |

The electromotive force increased gradually with the strength of the solution up to 0·06565 grain of the anhydrous acid, then remained constant until 0·10314 grain had been added, and then increased up to the strongest solution employed. The total increase of electromotive force was 0·3688 volt. The smallest proportion of the anhydrous acid required to disturb the balance of the couple lay between 1 part in 9,300,000 and 9,388,185 parts of water.

Table VI.—Bromine in 465 grains of Water at 12·5° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 20·10 | 1·9746 | 9·84 | 1·9089 |
| 18·39 | 1·9603 | 8·13 | 1·8974 |
| 16·68 | 1·9517 | 6·42 | 1·8775 |
| 14·97 | 1·9403 | 4·71 | 1·8715 |
| 13·26 | 1·9317 | 3·00 | 1·8743 |
| 11·55 | 1·9203 | | |

'he strongest of these solutions was a saturated one. The electro-ive force first decreased and then increased almost regularly with strength of the liquid up to the saturation point. The total unt of increase was 0·13 volt.

Table VII.—Ditto at 16° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 3·0 | 1·8746 | 1·5 | 1·7400 |
| 2·85 | 1·8173 | 1·35 | " |
| 2·7 | 1·7973 | 1·2 | " |
| 2·55 | 1·7887 | 1·05 | 1·7229 |
| 2·4 | 1·7687 | 0·9 | 1·7200 |
| 2·25 | 1·7573 | 0·75 | 1·7172 |
| 2·1 | " | 0·6 | 1·7027 |
| 1·95 | 1·7458 | 0·45 | " |
| 1·8 | " | 0·3 | " |
| 1·65 | " | 0·15 | " |

'y gradually increasing the strength of the liquid, the electro-ive force at first remained uniform, then increased, remained form again, then gradually increased, finally at a rapid rate. The l increase was 0·1719 volt.

Table VIII.—Ditto at 13·7° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·0004 | 1·2888 | 0·0001235 | 1·1658 |
| 0·0003005 | " | 0·000084 | 1·1515 |
| 0·000321 | 1·2802 | 0·0000445 | 1·1086 |
| 0·0002815 | 1·2745 | | |
| 0·000242 | 1·2459 | 0·0000081 | 0·937 |
| 0·0002025 | 1·2316 | 0·000005 | 0·9084 |
| 0·000163 | 1·1944 | Water. | " |

By regularly increasing the strength of the solution, the electromotive force at first increased very rapidly, then with decreasing rapidity, and finally remained uniform. The total increase was 0·38 volt. The smallest proportion of bromine required to upset the balance lay between 1 in 77,500,000 and 84,545,000 parts of water.

With each of these substances, and with all others which I have examined, a gradual and regular increase of strength of the solution from the weakest up to a saturated one, was attended by a more or less irregular change of electromotive force.

By plotting the quantities of dissolved substance as ordinates to the electromotive forces as abscissæ, each substance or mixture of substances in every case yielded a different curve of variation of electromotive force by uniformly changing the strength of its solution. With a given voltaic couple at a given temperature, the curve was constant and characteristic of the substance. As the least addition of a soluble foreign substance greatly changed the "minimum-point," and altered the curve of variation of potential, both the curve and the minimum proportion of a substance required to upset the voltaic balance may probably be used as tests of the chemical composition of the substance, and as means of examining its state of combination when dissolved. By varying the strength of the solution at each of the metals separately, a curve of change of potential was obtained for each positive metal, but not for every negative one.

## III. "Influence of the Chemical Energy of Electrolytes upon the 'Minimum Point' and Change of Potential of a Voltaic Couple in Water." By G. Gore, F.R.S. Received June 7, 1888.

In a communication to the Royal Society, May 3rd, 1888, on "The Effect of Chlorine upon the Electromotive Force of a Voltaic Couple," and in a subsequent one on "The Minimum Point of Change of Potential of a Voltaic Couple," I have shown that by opposing to each other two currents of equal electromotive force from two perfectly similar couples of magnesium-platinum or zinc-platinum in distilled water, and gradually adding to one of the cells sufficiently minute quantities of a suitable substance, such as chlorine, hydrochloric acid, or a soluble salt, &c., the voltaic balance *is not disturbed* until a certain definite proportion of the substance *has been added*, and that the proportion required to be added is *excessively* small (about 1 in 17,000 millions) in the case of chlorine

, magnesium-platinum couple, and extremely different with substances.

he present paper my object is to describe a few similar experi- made to examine the influence of liquids of different chemical sition, upon this phenomenon and upon the degrees of electro- force produced by further additions of the substances. All lutions were made with distilled water, and the substances yed were of considerable degree of purity. The voltaic cell ed in each case of zinc and platinum in distilled water, and its motive force was balanced by that of a suitable thermo-electric see 'Proceedings of the Birmingham Philosophical Society,' p. 130), and the measurements made under that condition.

electromotive force of a zinc-platinum couple in ordinary ed water at 16° C. is about 1·088 volt; provided the zinc is free xide, and the platinum contains no absorbed hydrogen. The ce of hydrogen (not removable by rubbing but removable by g to redness) may reduce the electromotive force to 0·91 volt, film of oxide upon the zinc may reduce it 1 or 2 per cent., carbonic acid absorbed by the water from the air, &c., may se it about 2 per cent. In all cases, therefore, where very measurements of electromotive force are necessary, these cir- ances have to be considered. In the present case the measure- are sufficiently accurate for the purposes intended.

eries of measurements were made with a zinc-platinum couple ter, adding uniform quantities of hydrochloric acid up to grain per 465 grains of water, and heating the platinum to s previous to each measurement. The variations of electro- force obtained were nearly the same as when the platinum was ated, the only material difference being that the electromotive hroughout was about 0·10 volt higher.

following are the results of the experiments made upon the ce of the chemical energy of the liquid. The numbers are ted for the influence of hydrogen absorbed by the platinum.

Table I.—$KIO_3$ in 465 grains of Water at 15° C.

| ins. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| ·05 | 1·40586 | 22·05 | 1·26 | 7·05 | 1·1456 |
| ·05 | 1·36296 | 19·05 | 1·2428 | 4·05 | 1·1313 |
| ·05 | 1·3172 | 16·05 | 1·2085 | 1·05 | 1·1370 |
| ·05 | 1·2829 | 13·05 | 1·2028 | 0·94 | 1·0884 |
| ·05 | 1·2743 | 10·05 | 1·14 | water | „ |

*strongest solution* employed was a saturated one. Four *t solutions, each* weaker than 0·94 grain, gave the same

electromotive force as water. The least proportion of the iodate necessary to upset the balance lay between 1 part in 443 and 494 parts of water. The increase of electromotive force by increased strength of the solution was nearly regular, as may be seen by plotting the quantities of substance as ordinates to the electromotive forces as abscissæ. In order to remove any trace of free iodine, the iodate was previously kept at 100° C. during one hour; it was then perfectly white and free from odour.

Table II.—$KBrO_3$ in 465 grains of Water at 14° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 19·5 | 1·2886 | 12 | 1·260 | 4·5 | 1·3344 |
| 18·0 | 1·2743 | 10·5 | " | 3·0 | 1·3000 |
| 16·5 | " | 9 | 1·3344 | 1·5 | 1·2600 |
| 15 | 1·2772 | 7·5 | " | .. | .. |
| 13·5 | 1·2972 | 6 | " | .. | .. |

The strongest solution was a saturated one.

Table III.—Ditto at 15° C.

| Grains. | Volts. |
|---|---|
| 1·5 | 1·260 |
| 1·35 | 1·117 |
| 1·2066 | 1·0884 |
| water | " |

Eight other solutions, all of different strengths below 1·2006, gave the same electromotive force as water. The smallest proportion of bromate required to upset the balance lay between 1 in 344 and 387 parts of water. The increase of electromotive force by increase of strength of the solution was extremely irregular.

The effects obtained with solutions of potassic chlorate have already been given in the paper on "The Change of Potential of a Voltaic Couple by Variation of Strength of its Liquid." The smallest proportion of the salt required to disturb the voltaic balance lay between 1 in 221 and 258 parts of water. Three solutions, each weaker than 1·8 grain in 465 grains of water, viz., 0·09, 0·009, and 0·0009 grain, gave the same electromotive force as water.

The following table shows the results obtained with this group of salts:—

Table IV.

[odate, minimum point of change lay between 1 in 443 and 494.
Bromate, „ „ „ 1 „ 344 „ 384.
Chlorate, „ „ „ 1 „ 221 „ 258.

The minimum points of change of these three salts constitute a ies indicating a gradation of degree of chemical union of the gative constituent of the salt with its base, feeblest in the iodate, ermediate with the bromate, and strongest in the chlorate. The re feebly united the negative constituent, the smaller was the oportion of the salt required to disturb the voltaic balance.

Table V.—KI in 465 grains of Water at 15° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 762 | 1·0584 | 727 | 1·1252 | 692 | 1·1728 |
| 755 | 1·0727 | 720 | 1·1442 | 685 | „ |
| 748 | 1·0784 | 713 | 1·1585 | 678 | „ |
| 741 | 1·0899 | 706 | 1·1728 | .. | .. |
| 734 | 1·2071 | 699 | „ | .. | .. |

The strongest solution was a saturated one.

Table VI.—Ditto at 13° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 678 | 1·1728 | 426 | 1·1556 | 174 | 1·1556 |
| 594 | 1·1899 | 342 | „ | 90 | „ |
| 510 | 1·1556 | 258 | „ | 6 | „ |

Table VII.—Ditto at 14° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 6·00 | 1·1556 | 4·89 | 1·0584 | 3·69 | 1·0584 |
| 5·49 | 1·1442 | 4·29 | „ | 3·09 | „ |

Table VIII.—Ditto at 19° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 3·0 | 1·0497 | 1·68 | 1·0669 | 0·36 | 1·0697 |
| 2·67 | 1·0583 | 1·35 | 1·0583 | 0·03 | 1·0716 |
| 2·34 | 1·0697 | 1·02 | 1·0697 | 0·027 | 1·0812 |
| 2·01 | 1·0726 | 0·69 | " | water | " |

The great solubility of the salt rendered several groups of measurements necessary in order to include the entire range of solution. The salt was odourless and colourless, but slightly alkaline. The smallest proportion of the iodide necessary to change the balance lay between 1 in 15,500 and 17,222 parts of water. The variation of electromotive force with strength of solution was very irregular. The greatest electromotive force was with a solution containing from 680 to 700 grains of the salt.

Table IX.—KBr in 465 grains of Water at 12·5° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 273 | 1·1442 | 153 | 1·2457 | 33 | 1·230 |
| 243 | 1·1771 | 123 | 1·2314 | 3 | 1·2317 |
| 213 | 1·2314 | 93 | 1·1485 | .. | .. |
| 183 | 1·2171 | 63 | 1·230 | .. | .. |

The salt was well crystallised, dry, odourless, and neutral to test-paper. The strongest solution of it was a saturated one.

Table X.—Ditto at 9° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 0·03 | 1·2872 | 0·01668 | 1·2443 | 0·00336 | 1·087 |
| 0·02667 | 1·2729 | 0·01335 | 1·3015 | water | " |
| 0·02334 | 1·2529 | 0·01001 | 1·2872 | .. | .. |
| 0·02001 | 1·2443 | 0·00669 | 1·1871 | .. | .. |

Six different strengths of solution, each weaker than 0·0036, gave the same electromotive force as water. The smallest proportion of the salt which upset the balance lay between 1 part in 66,428 and 67,391 parts of water.

Table XI.—KCl in 465 grains of Water at 12° C.

| rains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 147 | 1·30436 | 93 | 1·30436 | 39 | 1·30436 |
| 129 | „ | 75 | „ | 21 | „ |
| 111 | „ | 57 | „ | 3 | „ |

ie strongest solution was a saturated one. Four other solutions veen those of 129 and 147 grains were tried, but they all gave 436 volt. The abscissæ of the electromotive forces in this table ied a straight line.

Table XII.—Ditto at 8° C.

| rains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 003 | 1·3056 | 0·001335 | 1·2014 | 0·000224 | 1·087 |
| 002667 | 1·2671 | 0·001001 | 1·1728 | 0·000112 | „ |
| 002334 | 1·2043 | 0·000669 | 1·1442 | water | „ |
| 002001 | 1·2443 | 0·000660 | 1·087 | .. | .. |
| 001668 | 1·230 | 0·000336 | „ | .. | .. |

he smallest proportion of the salt necessary to disturb the voltaic nce lay between 1 in 695,067 and 704,540 parts of water. The ation of electromotive force in these solutions was not uniform.
he following table shows the proportions of these three salts ired to upset the balance :—

Table XIII.

| | | | | | |
|---|---|---|---|---|---|
| Iodide, between | 1 in | 15,500 | and | 17,222 | parts of water. |
| Bromide „ | 1 „ | 66,428 | „ | 67,391 | „ „ |
| Chloride „ | 1 „ | 695,067 | „ | 704,540 | „ „ |

y comparing these numbers with those in Table IV, it will be eived that each of the haloid salts acted much more powerfully either of the oxygen ones, and that the order of degrees of ity in the two series was reverse.
uspecting a decomposition of the chloride solution by the couple, ided a solution of 8 grains of the salt per ounce of water into equal portions in two glass vessels, then immersed a piece of zinc in one portion, and a second piece of the same wire in contact a piece of platinum in the other, and set the vessels aside. In t *24 hours the liquid* containing the couple was distinctly alkaline,

whilst the other remained neutral. I have examined this phenomenon further.)

The three halogens of the salts were now employed separately. A saturated solution of iodine was prepared by digesting a weighed amount of that substance in a known volume of hot distilled water in a stoppered glass flask with continual agitation; it contained 1 part of dissolved iodine in 3516 parts of water.

Table XIV.—Iodine in 465 grains of Water at 13·5° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 0·1320 | 1·374 | 0·0546 | 1·374 | 0·00015 | 1·0894 |
| 0·1191 | " | 0·0417 | " | 0·000132 | 1·088 |
| 0·1062 | " | 0·0288 | " | 0·000075 | " |
| 0·0933 | " | 0·0159 | " | water | " |
| 0·0804 | " | 0·003 | 1·2659 | .. | .. |
| 0·0675 | " | 0·0003 | 1·1372 | .. | .. |

Four other solutions, weaker than 0·000075 grain, gave each 1·088 volt. The minimum proportion of iodine required to upset the balance lay between 1 part in 3,100,000 and 3,521,970 parts of water. Except in very weak solutions, variations of strength of the liquid had no effect upon the electromotive force.

The effects obtained with bromine have already been given in the paper on "The Change of Potential of a Voltaic Couple by Variations of Strength of its Liquid." The smallest proportion of that substance required to disturb the balance lay between 1 part in 77,500,000 and 84,545,000 parts of water. By dissolving bromine in the proportions of 0·000075, 0·00015, 0·000165, and 0·00018 grain respectively in 13,950 grains of distilled water at 12°C., the three first of these solutions gave the same potential with zinc-platinum as that given by water, whilst the fourth gave 0·0064 volt greater.

Table XV.—Chlorine in 465 grains of Water at 11° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 1·0695 | 2·312 | 0·6537 | 2·3349 | 0·2379 | 2·3405 |
| 1·0002 | 2·3206 | 0·5844 | 2·332 | 0·1686 | 2·2862 |
| 0·9309 | 2·3349 | 0·5151 | " | 0·0993 | 2·2805 |
| 0·8616 | " | 0·4458 | 2·3092 | 0·0300 | 2·226 |
| 0·7923 | " | 0·3765 | 2·3891 | .. | .. |
| 0·723 | 2·2692 | 0·3072 | 2·2577 | .. | .. |

The strongest solution was about three-fourths saturated.

Table XVI.—Ditto at 13° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 0·03 | 2·2261 | 0·018 | 1·8457 | 0·006 | 1·7748 |
| 0·027 | 2·1317 | 0·015 | 1·84 | 0·003 | " |
| 0·024 | 2·0459 | 0·012 | 1·817 | 0·0000003 | 1·18 |
| 0·021 | 1·9716 | 0·009 | 1·7748 | water | 1 088 |

Table XVII.—Ditto at 13° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 0·003 | 1·7748 | 0·0000937 | 1·4173 | 0·00000293 | 1·26 |
| 0·0015 | 1·6604 | 0·0000468 | 1·403 | 0·000001464 | 1·2457 |
| 0·00075 | 1·6318 | 0·0000234 | 1·3887 | 0·000000732 | 1·1799 |
| 0·000375 | 1·5346 | 0·00001172 | 1·3744 | water | 1·0884 |
| 0·0001875 | 1·4316 | 0·00000585 | 1·3605 | .. | .. |

Table XVIII.—Ditto in 13,950 grains of Water at 11° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·00001247 | 1·1313 | 0·00000713 | 1·0884 |
| 0·00001104 | 1·0998 | 0·000003565 | " |
| 0·00001069 | 1·0884 | water | " |
| 0·0000089 | " | .. | .. |

The mode by which the chlorine-water was prepared and its strength ascertained has been already described ('Roy. Soc. Proc.,' vol. 44, 1888, p. 151, 'Nature,' vol. 38, p. 117). The minimum proportion of chlorine necessary to upset the balance was found more nearly by adding very small quantities of an exceedingly dilute solution of it to the water until the required strength was attained, thus avoiding the risk of error attending more numerous dilutions. The proportion lay between 1 in 1264 million and 1300 million parts of water. The variation of electromotive force by uniform increase of the strength of the solution was irregular.

The following are the minimum proportions of iodine, bromine, and chlorine, arranged for comparison :—

Table XIX.—Minimum Proportions.

| | | | | | |
|---|---|---|---|---|---|
| Iodine, between | 1 | part in | 3,100,000 | and | 3,521,970 |
| Bromine | ,, | 1 ,, | 77,500,000 | ,, | 84,545,000 |
| Chlorine | ,, | 1 ,, | 1264,000,000 | ,, | 1300,000,000 |

This series of numbers suggests a quantitative relation of the "minimum proportions" to the atomic and molecular weights of the substances.

On comparing these numbers with those of the two previous groups of bodies, we find that the proportion of substance required to upset the voltaic balance was largest with the oxygen salts, intermediate with the haloid ones, and least with the free elementary bodies. It was smaller the greater the degree of chemical energy of the substance; thus it was about 400 times less with chlorine than with iodine. And it was smaller the greater the degree of freedom to exert that energy; thus it was about 5,416,000 times smaller with free chlorine than with potassic chlorate, or 1,570,000 times less than with the combined chlorine of the chlorate; and about 185 times smaller than with potassic chloride, or 88 times less than with the combined chlorine of that salt.

At the lowest potentials, the rate of increase of electromotive force per grain of substance is usually larger the smaller the proportion of substance necessary to disturb the potential. Iodine is an exception to this, but probably only an apparent one, because on substituting magnesium for the zinc, the addition of iodine caused an increase of potential as usual.

The curve of variation of potential was different with the solution of each substance, and was apparently characteristic of the body in each case; and a great number of such representative curves might be obtained by change of strength of solution, in nearly all electrolytes, with a zinc-platinum or other voltaic couple.

## IV. "The Electric Organ of the Skate. The Electric Organ of *Raia radiata*." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Professor J. BURDON SANDERSON, F.R.S. Received June 6, 1888.

(Abstract.)

The first part of this paper is chiefly devoted to a comparison of the electric organs of *Raia radiata*, *R. batis*, and *R. circularis*. It is shown that the organ in the species *radiata* differs in many respects from the organ in the two other species, and that an exhaustive

study of its structure and development is likely to throw considerable light on the nature of electric organs generally, and also on the structure of the motor plates of muscles. While *R. batis* may reach a length of over 180 cm., *R. radiata* seldom measures more than 45 cm. from tip to tip, and is thus only about half the size of a large *R. circularis*. In *R. radiata* the electric organ is absolutely and relatively extremely small. In *R. batis* the electric organ may be 60 cm. in length and 7 cm. in circumference at the centre, and extend from the skin to the vertebral column, but in an adult *R. radiata* the organ is seldom over 13 cm. in length and 8 mm. in circumference, and the posterior two-thirds is confined to a narrow cleft between the skin and the great lateral muscles of the tail. Further, the organ of *R. radiata* consists of minute shallow cups, which only remotely resemble the large well-formed electric cups of *R. circularis*. In the latter species the various layers of the electric cup are readily comparable to the more important layers of the electric disk of *R. batis*, but in *R. radiata* the electric cup is little more than a muscular fibre, with one end expanded and slightly excavated to support a greatly enlarged motor plate, in which terminate numerous nerve-fibres. The striated layer of *R. batis* and *R. circularis*, which consists of characteristic lamellæ having an extremely complex arrangement, is entirely absent from *R. radiata*, the electric layer is indistinct, and instead of a thick richly nucleated cortex, the cup is merely invested by a slightly thickened sarcolemma. Further, the tissue forming the shallow, thick-walled cup, both in its appearance and consistency, closely resembles an ordinary muscular fibre, while the long stem usually remains distinctly striated to its termination.

In the second part of the paper an account is given of the development of the electric cups of *R. radiata*. It is shown that the rate of development compared with *R. circularis*, but more especially with *R. batis*, is extremely slow. The young *R. radiata* is nearly double the size of the *R. batis* embryo before the muscular fibres reach the "club" stage, and the long nearly uniform clubs, instead of at once developing into rudimentary cups as is the case in *R. batis*, assume the form of large Indian clubs. When the young skate reaches a length of about 35 cm., the long secondary (Indian) clubs begin to expand anteriorly, and this expansion continues until a fairly well moulded cup, mounted on a long delicate stem is produced. But the process of conversion is scarcely completed when the skate has reached a length of 40 cm., *i.e.*, when it has nearly reached its full size, for in the species *radiata* a length of 50 cm. is seldom if ever attained.

The cup-stage having been eventually reached, the stem, which for a time may still increase in length, is often compressed by two or more cups being closely applied together, and part of the rim of the cup may be slightly everted or projected forwards, but even in the

largest specimens of *R. radiata* examined there was never any indication of retrogressive changes.

The small size of the electric organ, together with the shallowness of the minute cups of which it consists, seems at first to indicate that in *R. radiata* we have an electric organ in the act of disappearing. But when the organ of the species *radiata* is carefully compared with the organ of the species *batis* and *circularis*, the evidence seems to point in an opposite direction, and the view that the cups of *R. radiata* are in process of being elaborated into more complex structures, such as already exist in *R. circularis*, is apparently confirmed by the developmental record. Were the electrical organ of *R. radiata* a mere vestige of a larger structure which formerly existed, we should expect to find the motor (electric) plate incomplete, or only occupying a portion of the electric cup and the nerves proceeding to it, either few in number or undergoing degenerative changes. But instead of this we have a relatively large bunch of extremely well-developed nerves proceeding to the electric plate, which is not only complete, but extends some distance over the rim of the cups. Further, there is no indication of the walls of the cup having ever consisted of extremely complex lamellæ, such as we have in *R. circularis*. They consist of a nearly solid mass of muscular tissue, scarcely to be distinguished from the unaltered adjacent muscular fibres. The electric cup of *R. radiata* may in fact, when its structure alone is considered, be said to be a muscular fibre which has been enlarged at one end to support a greatly overgrown motor plate. But the development of the electric cups is even more suggestive than their structure. Had the muscular fibres in *R. radiata* assumed the form of clubs before the young skate escaped from the egg capsule; had the clubs been rapidly transformed into electric cups; and had the cups soon after reaching completion begun to disappear, the evidence in favour of degeneration would have been complete. But, as has been indicated, the conversion of the muscular fibres into an electric organ is late in beginning, and the clubs having appeared, pass slowly through a prolonged series of intermediate stages before they eventually assume the cup form. Further, as has already been mentioned, in the largest specimens of *R. radiata* examined no evidence was found of retrogressive changes, either in the cup proper, or in the numerous nerves passing to its electric plate. Hence it may be inferred that the electric organ of *R. radiata*, notwithstanding its apparent uselessness and its extremely small size, is in a state of progressive development.

V. "On certain Definite Integrals. No. 16." By W. H. L. RUSSELL, F.R.S. Received May 31, 1888.

In these papers I have considered incidentally the advantages gained by differentiating and integrating with regard to the quantities which are independent of the leading variable. In the present communication I enter upon this subject more systematically, as it evidently admits of wide extension.

$$\int_0^\infty \frac{\epsilon^{-a^2x^2}dx}{m^2+x^2} = \frac{\pi\epsilon^{m^2a^2}}{m}\int_{am}^\infty \epsilon^{-x^2}\,dx,$$

$$\int_0^\pi \{\epsilon^{3i\theta}\phi\epsilon^{2i\theta} + \epsilon^{-3i\theta}\phi\epsilon^{-2i\theta}\}\frac{d\theta}{\sin\theta} = 2i\pi\phi 0.$$

(See No. 88 of this series.)

$$\int_0^{\frac{\pi}{2}} d\theta\cos\theta\left\{\frac{\epsilon^{i\tan\theta+\theta}}{1-\mu\epsilon^{i\sin\theta}}\cdot\phi\frac{\epsilon^{i\tan\theta}}{1-\mu\epsilon^{i\tan\theta}}+\right.$$

$$\left.\frac{\epsilon^{-i(\tan\theta+\theta)}}{1-\mu\epsilon^{-i}\tan\theta}\phi\frac{\epsilon^{-i\tan\theta}}{1-\mu\epsilon^{-i\tan\theta}}\right\} = \frac{\pi}{2}\cdot\frac{1}{\epsilon-\mu}\phi\frac{1}{\epsilon-\mu}.$$

This is obtained from integral 21 of this series, where, however, in the denominators of the integral cos tan $\theta$ is misprinted cos $\theta$.

$$\int_0^{\frac{\pi}{2}} dx\left\{\frac{\cos x\epsilon^{i(\tan x+x)}}{1-a\cos x\epsilon^{ix}}\phi\left(\frac{\cos x\epsilon^{ix}}{1-a\cos x\epsilon^{ix}}\right)\right.$$

$$\left.+\frac{\cos x\epsilon^{-i(\tan x+x)}}{1+a\cos x\epsilon^{-ix}}\phi\left(\frac{\cos x\epsilon^{-ix}}{1-a\cos x\epsilon^{-ix}}\right)\right\} = \frac{\pi}{\epsilon^c}\frac{1}{2-a}\phi\frac{1}{2-a}.$$

See integral 22.

$$\int_0^{\frac{\pi}{2}}\frac{d\theta}{\sin\theta}\{\epsilon^{i\theta}\phi(\cos\theta\epsilon^{i\theta}) + \epsilon^{-i\theta}\phi(\cos\theta\epsilon^{-i\theta})\} = \pi i\phi(1).$$

See Abel, 'Œuvres Complètes,' vol. 2, page 88.

$$\int_0^{\frac{\pi}{2}} d\theta\,\{\cos^2\theta\,\epsilon^{2x\cos^2\theta}\,\epsilon^{ix\sin 2\theta}\,\phi\,(2\cos\theta\,\epsilon^{2\theta})$$

$$+\cos^2\theta\,\epsilon^{2x\cos^2\theta}\,\epsilon^{-ix\sin 2\theta}\,\phi\,(2\cos\theta\,\epsilon^{-i\theta})\}$$

$$=\pi(2\phi(1)-\phi'(1)\epsilon^x+\phi(1)\,.\,x\epsilon^x).$$

*See integral* 116 of this series.

Again, since

$$\int_0^\pi d.\theta \frac{f\epsilon^{\theta i} + f\epsilon^{-\theta i}}{1 - 2\alpha\cos\theta + \alpha^2} = \frac{2\pi}{1-\alpha^2} f(\alpha),$$

we may write if $f(\alpha)$ be a rational fraction

$$\int_0^\pi d\theta \frac{f\epsilon^{\theta i} + f\epsilon^{-\theta i}}{\epsilon^{-\theta i} - \epsilon^{\theta i}} \left(\frac{1}{\epsilon^{\theta i} - a} - \frac{1}{\epsilon^{-\theta i} - a}\right) = \Sigma \frac{M}{\mu - a},$$

and, therefore,

$$\int_0^\pi d\theta \frac{f\epsilon^{\theta i} - f\epsilon^{-\theta i}}{\epsilon^{-\theta} - \epsilon^{\theta i}} \left\{\frac{1}{\epsilon^{\theta i} - a} \phi \frac{1}{\epsilon^{\theta i} - a}\right.$$

$$\left. - \frac{1}{\epsilon^{-\theta i} - \alpha} \phi \frac{1}{\epsilon^{\theta i} - a}\right\} = \Sigma \frac{M}{\mu - a} \phi \frac{1}{\mu - a}.$$

We know that

$$\int_0^\infty \frac{dx}{1+x^2} \frac{1}{1 - 2a\cos x + a^2} = \frac{\pi}{2} \frac{1}{1-a^2} \frac{\epsilon + a}{\epsilon - a},$$

that is—

$$\int_0^\infty \frac{dx}{1+x^2} \cdot \frac{1}{\epsilon^{-ix} - \epsilon^{ix}} \left\{\frac{1}{\epsilon^{ix} - a} - \frac{1}{\epsilon^{-ix} - a}\right\}$$

$$= \frac{\pi}{4} \cdot \frac{\epsilon + 1}{\epsilon - 1} \cdot \frac{1}{1-a} + \frac{\pi}{4} \frac{\epsilon - 1}{\epsilon + 1} \cdot \frac{1}{1+a} - \frac{\pi\epsilon}{\epsilon^2 - 1} \cdot \frac{1}{\epsilon - a}.$$

Hence we have

$$\int_0^\infty \frac{dx}{1+x^2} \cdot \frac{1}{\epsilon^{-ix} - \epsilon^{ix}} \left\{\frac{1}{\epsilon^{ix} - a} \phi \frac{1}{\epsilon^{ix} - a}\right.$$

$$\left. - \frac{1}{\epsilon^{-x} - a} \phi \frac{1}{\epsilon^{ix} - a}\right\} = \frac{\pi}{4} \cdot \frac{\epsilon + 1}{\epsilon - 1} \cdot \frac{1}{1-a} \phi \frac{1}{1-a}$$

$$+ \frac{\pi}{4} \frac{\epsilon - 1}{\epsilon + 1} \cdot \frac{1}{1+a} \phi \frac{-1}{1+a} - \frac{\pi\epsilon}{\epsilon^2 - 1} \cdot \frac{1}{\epsilon - a} \cdot \phi \frac{1}{\epsilon - a}.$$

Let now $\phi(x) = \left\{x^3 \frac{d^3}{dx^3} + 3x \frac{d^2}{dx^2} + \frac{d}{dx}\right\} \chi x$

be a relation connecting the two functions $\phi(x)$ and $\chi(x)$.

Then $x\phi(x) = \left(x \frac{d}{dx}\right)^3 \chi(x)$, and we may put $x\phi(x) = A_0 x + A_1 x^2 + \ldots + A_n x^{n+1} + \ldots$, then making use of the symbol $\left(x \frac{x}{dx}\right)^{-3}$, we shall obtain

$$\chi(x) = x\left(A_0 + A_1\frac{x}{2^3} + A_2\frac{x^2}{3^3} + \ldots\right).$$

But
$$\int_0^1 v^{n-1}\left(\log_e\frac{1}{v}\right)^2 dv = \frac{1}{n^3}\,\Gamma(3)$$

Therefore we shall find

$$\chi(x) = \frac{x}{2}\int_0^1 dv\left(\log_e\frac{1}{v}\right)^2(A_0 + A_1vx + \ldots)$$

or
$$\int_0^1 dv\left(\log_e\frac{1}{v}\right)^2\phi\,(vx) = \frac{2\chi(x)}{x}.$$

As $x\phi\,(x)$ or $\left(x\,\frac{d}{dx}\right)^3\chi\,(x)$ can have no constant term, all the terms of the expanded form of $\left(x\,\frac{d}{dx}\right)^{-3}x\phi\,(x)$ are suitable for the application of the definite integral.

Again let
$$\phi(x) = \left(x^2\frac{d^3}{dx^3} + 9x\,\frac{d^2}{dx^2} + 15\,\frac{d}{dx}\right)\lambda(x)$$

then
$$x\phi(x) = \left(x^{-3}\frac{d}{dx}x^3\frac{d}{dx}x^3\frac{d}{dx}\right)\chi(x)$$

so if
$$\chi(x) = \frac{d^{\,-1}}{dx}\,x^{-3}\,\frac{d^{\,-1}}{dx}\,x^{-3}\,\frac{d^{\,-1}}{dx}\,x^3\;.\;x\phi(x)$$

and
$$x\phi\,(x) = A_0x + A_1x^2 + \ldots + A_{n-1} - x^n + \ldots$$

we find
$$\chi(x) = \frac{A_0x}{1\,.\,3\,.\,5} + \frac{A_1x^2}{2\,.\,4\,.\,6} + \cdots + \frac{A_{n-1}x^n}{n(n+2)(n+4)} + \cdots$$

$$= \frac{1}{2^3}\left\{\frac{A_0\Gamma\frac{1}{2}\,.\,x}{\Gamma\frac{7}{2}} + \frac{A_1x^2\Gamma(1)}{\Gamma(4)} \cdots + \frac{A_{n-1}x^n\Gamma\frac{n}{2}}{\Gamma(\frac{n}{2}+3)} + \cdots\right\}$$

$$= \frac{1}{2^4}\left\{\frac{A_0\Gamma\frac{1}{2}\Gamma(3)}{\Gamma\frac{7}{2}}x + \cdots + \frac{A_{n-1}\Gamma\frac{n}{2}\Gamma3}{\Gamma(\frac{n}{2}+3)}\,x^n + \right\}$$

$$= \frac{x}{2^4}\left\{\int_0^1\frac{dv}{\sqrt{v}}\,(1-v)^2(A_0 + A_1x\sqrt{v} + \ldots)\right\}$$

$$= \frac{x}{2^4}\int_0^1\frac{dv}{\sqrt{v}}\,(1-v)^2\phi\,(x\sqrt{v})$$

and so
$$\int_0^1\frac{dv}{\sqrt{v}}\,(1-v)^2\phi\,(x\sqrt{v}) = \frac{2^4\chi(x)}{x}$$

*or if we please*
$$\int_0^1 du\,(1-u^2)^2\phi\,(xu) = \frac{2^3\lambda(x)}{x}.$$

If we put $\chi(x) = x^2 + x$ in this integral, we shall obtain a perfectly correct result.

I discovered the following integral some years ago. It may have been discovered before, although I have been unable to meet with it.

$$\int_0^{\frac{\pi}{2}} d\theta\, \theta\, (2\cos\theta)^{m-1} \sin(m + 2r + 1)\,\theta$$

$$= \pm\frac{\pi}{4}\cdot\frac{1\,.\,2\,.\,3\,\ldots\,.\,r}{m\,(m+1)\,(m+2)\ldots.(m+r)}.$$

From this may be deduced an enormous number of results, as will be at once apparent. I will write down two of them.

$$\int_0^{\frac{\pi}{2}} d\theta\,\theta\,\frac{\cos 5\theta\sin\theta + (1-x)\sin 5\theta\cos\theta}{x^2 + 2x + 2 + (x^2 + 2x)\cos 2\theta}$$

$$= \pi\left\{\frac{(x+2)^2}{4x^3}\log_e\left(1+\frac{x}{2}\right) - \frac{3x+4}{8x^2}\right\}.$$

Now let $\Theta_r = \cos^{n-r}\theta\sin(r+2)\,\theta.$

Then $$\int_0^{\frac{\pi}{2}}\theta\,\frac{\Theta_4 - 4\alpha\Theta_3 + 6\alpha^2\Theta_2 - 4\alpha^3\Theta_1 + \alpha^4\Theta_0}{((\alpha^2 - 2\alpha + 2) + (\alpha^2 - 2\alpha)\cos 2\theta)^4}\,d\theta$$

$$= \frac{\pi}{24}\cdot\frac{1}{8-4\alpha}.$$

The first integral was derived from the series $\frac{x^3}{1\,.\,2\,.\,3} - \frac{x^4}{2\,.\,3\,.\,4} + \ldots$, the second from $\frac{1\,.\,2\,.\,3}{1\,.\,2\,.\,3} + \frac{2\,.\,3\,.\,4}{2\,.\,3\,.\,4}\,\alpha + \ldots$

## VI. "On Meldrum's Rules for handling Ships in the Southern Indian Ocean." By Hon. RALPH ABERCROMBY, F.R. Met. Soc. Communicated by R. H. SCOTT, F.R.S. Received June 7, 1888.

(Abstract.)

The results of this paper may be summarised as follows:—

The author examines critically certain rules given by Mr. C. Meldrum for handling ships during hurricanes in the South Indian Ocean, by means both of published observations and from personal inspection of many unpublished records in the Observatory at Mauritius. The result confirms the value of Mr. Meldrum's rules; and the author then develops certain explanations, which have been partially given by Meldrum, adds slightly to the rules for handling

ships, and correlates the whole with the modern methods of meteorology.

As an example, a hurricane is taken which blew near Mauritius on February 11, 12, and 13, 1861, and the history of every ship to which the rules might apply is minutely investigated. The result, dividing Meldrum's rules shortly into three parts, is as follows:—

Rule 1. Lie to with increasing south-east wind till the barometer has fallen 6-10ths of an inch. Seven cases, rule right in every case.

Rule 2. Run to north-west when the barometer has fallen 6-10ths of an inch. Three cases, two failures, one success.

Rule 3. Lie to with increasing north-east or east wind and a falling barometer. Seven cases, rule right in every instance.

Rule 2 was exceptionally unfortunate in this case, as the path of the central vortex moved in a very uncommon and irregular manner. At the same time, in any case, it appears to be about equally hazardous to follow this rule or to remain hove to.

The following statements are then examined in detail:—

The shape of all hurricanes is usually oval, not circular. An elaborate examination is made of hurricanes on 60 different days, in 18 different tropical cyclones in various parts of the world, with the following results:—

1. Out of 60 days, cyclones were apparently circular on only four occasions, and then the materials are very scanty.

2. The shape was oval on the remaining 56 days, but the ratio of the longer and shorter diameter of the ovals very rarely exceeded 2 to 1.

3. The centres of the cyclones were usually displaced towards some one side. No rule can be laid down for the direction of displacement, and in fact the direction varies during the progress of the same cyclone. The core of a hurricane is nearly as oval as any other portion.

4. The longer diameter of the ovals may lie at any angle with reference to the path of the cyclone; but a considerable proportion lie nearly in the same line as the direction of the path.

5. The association of wind with the oval form is such that the direction of the wind is usually more or less along the isobars, and more or less incurved. This is the almost invariable relation of wind to isobars all over the world.

From an examination of the whole it is proved conclusively that *no rule is possible for determining more than approximately the position of the central vortex of a cyclone by any observations at a single station.*

The relation of a hurricane to the south-east trade is then discussed, and it is shown that there is always what may be called "a belt of intensified trade wind" on the southern side of a cyclone, while the hurricane *is moving westwards.* In this belt a ship experiences increasing *south-east winds and squalls* of rain, with a falling barometer, but is not

within the true storm field. The difficulties and uncertainties as to handling a ship in this belt are greatly increased by the facts that the longer diameter of the oval form of the cyclones usually lies east and west, and that there is no means of telling towards which side of the oval the vortex is displaced.

The greater incurvature of the wind in rear than in front of hurricanes in the Southern Indian Ocean is next considered, and then facts are collected from other hurricane countries confirmatory of Meldrum's rules for the Mauritius.

Knipping and Doberck in the China Seas find little incurvature of the wind in front, but much in rear of typhoons.

Mr. Wilson finds in the Bay of Bengal that north-east winds prevail over many degrees of longitude to the north, *i.e.*, in front of a cyclone; and this is analogous to the belt of intensified trade so characteristic of Mauritius hurricanes.

Padre Viñez finds at Havana that the incurvature of hurricane winds is very slight in front, and very great in rear.

The author then details further researches on the nature of cyclones, which bear on the rules for handling ships.

1. Indications derived from the form and motion of clouds. It is shown that the direction of the lower clouds is usually more nearly eight points from the bearing of the vortex than the surface wind; but as the direction varies with the height of the clouds, and as this height can only be estimated, this fact is not of much value.

2. Looking at the vertical succession of wind currents in the Southern Indian Ocean, if the march of the upper clouds over the south-east trade is more from the east, then the cyclone will pass to the north of the observer; but if the upper clouds move more from the south than the surface wind, then the hurricane will pass to the south of the observer.

3. As to the form and position of clouds; so soon as the upper regions commence to be covered, the direction in which the cirrus veil is densest gives approximately the bearing of the vortex. Later on, the characteristic cloud bank of the hurricane appears, and the greatest and heaviest mass of the bank will appear sensibly in the direction of the vortex.

The irregular motion of the centre of a cyclone is next discussed, and it is shown that the centre often twists and sways about, in some cases even describing a small loop.

From all the facts relative to the nature of cyclones adduced in this paper it is shown that the attempts which have been made—

1. To estimate the track of a cyclone by projection;

2. To estimate the distance of a ship from the vortex, either by taking into account the entire absolute fall, or by noting the rate of fall, can lead to no useful result.

A series of revised rules for handling ships in hurricanes in any part of the world is given. Comparing these rules with the older ones it will be remarked—

1. That the rule for finding approximately the bearing of the vortex is slightly modified.

2. That the great rules of the "laying to" tacks remain unaltered.

3. That the greatest improvement is the recognition of the position and nature of the belt of intensified trade wind on the dangerous side of a hurricane, where a ship experiences increasing wind, without change of direction, and a falling barometer. The old idea that such conditions show that a vessel is then *necessarily* exactly on the line of advance of a hurricane is erroneous. She may, but she need not be; and under no circumstances should she run till the barometer has fallen at least 6–10ths of an inch.

4. There are certain rules which hold for all hurricanes; but every district has a special series, due to its own local peculiarities. Those for the Southern Indian Ocean are given in this paper.

## VII. "Magnetic Properties of an Impure Nickel." By J. Hopkinson, F.R.S. Received June 9, 1888.

[Plates 2—13.]

The sample of nickel on which these experiments were made was supposed to be fairly pure when the experiments began. A subsequent analysis, however, showed its composition to be as follows :—

| | |
|---|---|
| Nickel | 95·15 |
| Cobalt | 0·90 |
| Copper | 1·52 |
| Iron | 1·05 |
| Carbon | 1·17 |
| Sulphur | 0·08 |
| Phosphorus | minute trace |
| Loss | 0·13 |
| | 100·00 |

The experiments comprise determinations of the curve of magnetisation at various temperatures, the magnetising force being increased, that is to say, they are confined to a determination of the ascending curve of magnetisation. The temperature was always produced by enclosing the object to be tested in a double copper casing with an air space between the two shells of the casing, and by heating the casing from without by a bunsen burner. The temperature was measured by determining the electrical resistance of a coil of copper wire. The copper was first roughly tested to ascertain that its

temperature coefficient did not deviate far from 0·0388 per degree centigrade of its resistance at 20° C.; I was unable to detect that the coefficient deviated from this value in either direction. The temperature may therefore be taken as approximately accurate.

The nickel had the form of a ring—fig. 1. On this ring was wound in one layer 83 convolutions of No. 27 B.W.G. copper wire carefully insulated with asbestos paper to serve as measurer of temperature and as secondary or exploring coil. Over this again, a layer of asbestos paper intervening, was wound a coil of 276 convolutions in five layers of No. 19 B.W.G. copper wire to serve as the primary coil.

The method of experiment was simply to pass a known current through the primary, to reverse the same and observe the kick on a ballistic galvanometer due to the current induced in the secondary. At intervals the secondary was disconnected, and its resistance was ascertained for a determination of temperature. Knowing the current it is easy to calculate the magnetising force, and knowing the constants of the galvanometer it is easy to calculate the induction per square centimetre. The practice was to begin by heating the ring to a temperature at which it ceased to be magnetic, then to lower the gas flame to a certain extent and allow the apparatus to stand for some time, half an hour or more, to allow the temperature to become steady, then determine the temperature, then rapidly make a series of observations with ascending force, lastly, determine the temperature again. The ring was next demagnetised by a series of reversals with diminishing currents. The flame was further lowered, and a second series of experiments was made. It was then assumed that the previous magnetisation would have a very small effect on any subsequent experiment. As the substance turned out to be far from pure nickel, it is not thought worth while to give actual readings. The results are given in the accompanying curves, Nos. 1 to 14, in which the abscissæ represent the magnetising forces per linear centimetre, the ordinates the induction per square centimetre, both in C.G.S. units. Curves 15 and 16 give the results of Professor Rowland* for pure nickel at the two temperatures at which he experimented. In curves 17 to 20 are given the inductions in terms of the temperature for stated intensities of the magnetising force, the ordinates being the inductions, the abscissæ the temperatures.

An inspection of these curves reveals the following facts:—

1. In my impure nickel much greater magnetising forces are required to produce the same induction than are required in Professor Rowland's pure nickel.

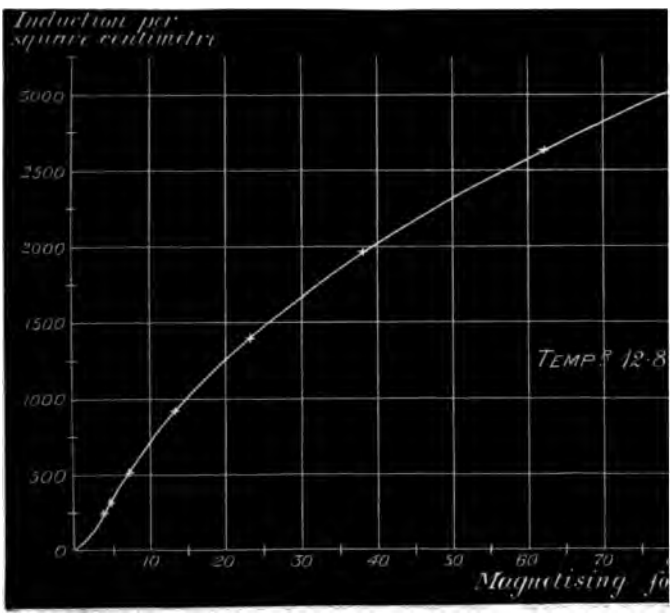
*2. The portion* of the curve which is concave upwards in my *sample is less* extensive and less marked than in his.

* 'Phil. Mag.,' November 1874.

Fig. 1.


6.5
51 dia
6.5
25.4

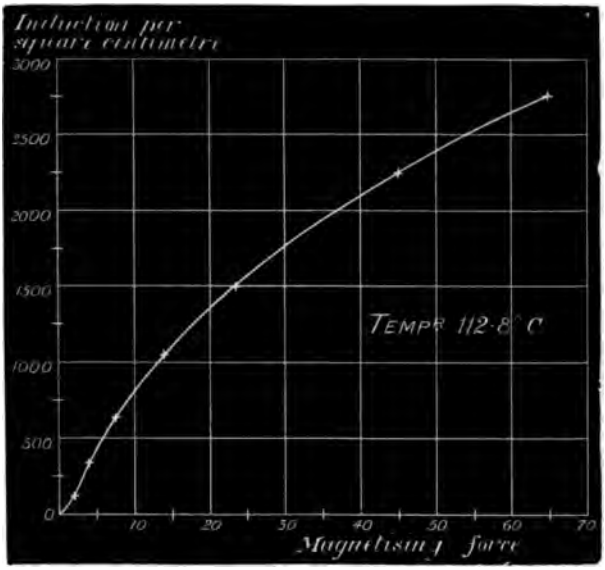
Curve I.


Induction per square centimetre
3000
2500
2000
1500
1000
500
0
10
20
30
40
50
60
70
Magnetising

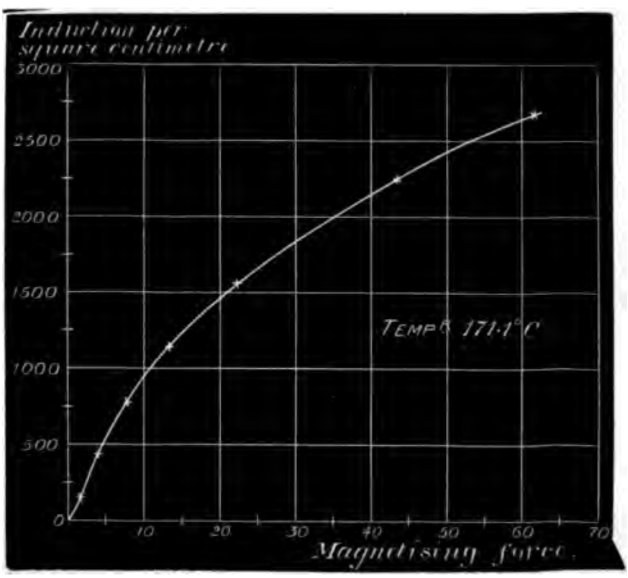
[redacted]



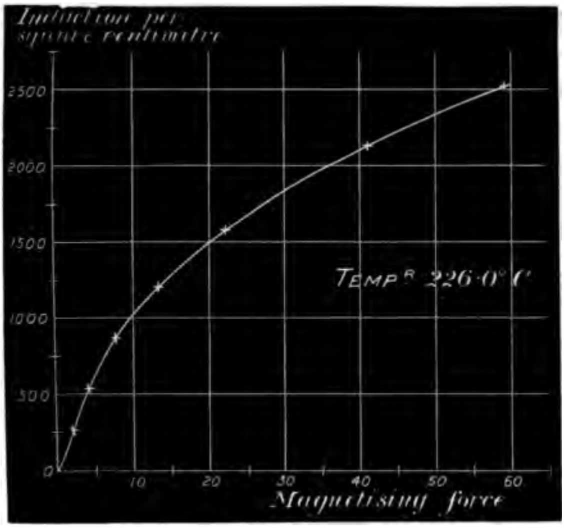
CURVE II.



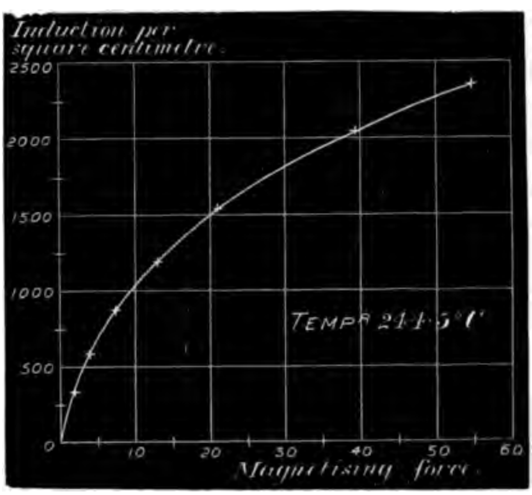

CURVE III.



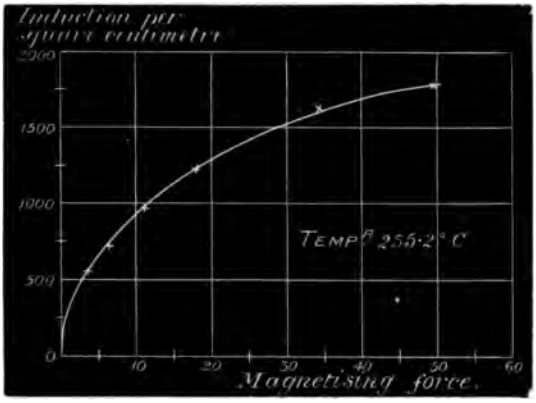
CURVE IV.



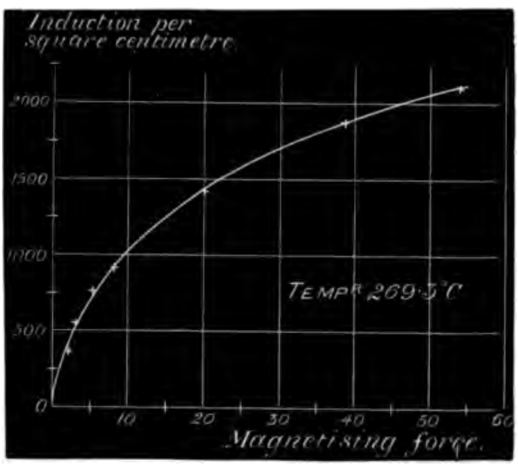

CURVE V.



CURVE VI.

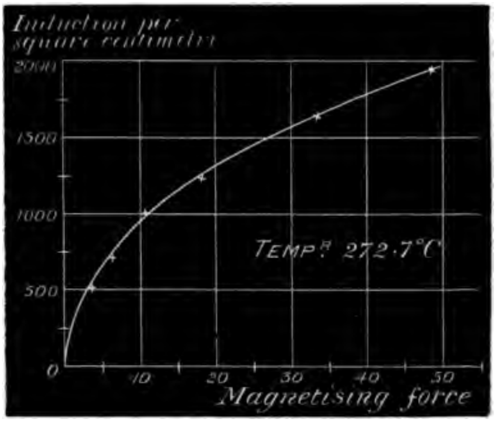



CURVE VII.

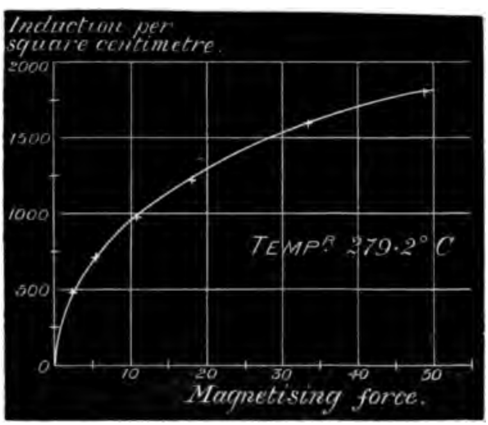


[redacted]

**Curve VIII.**

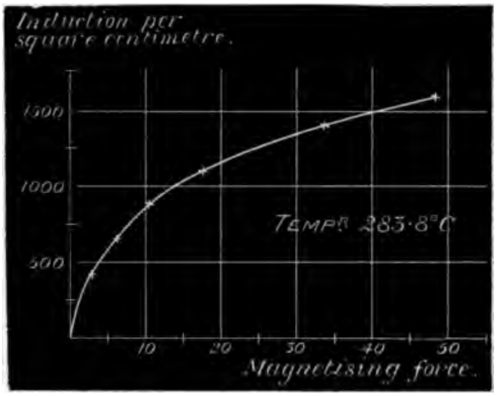



**Curve IX.**

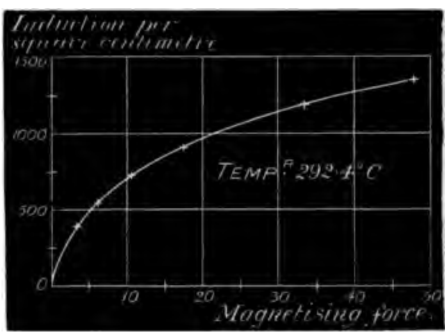


[Pl. 7.

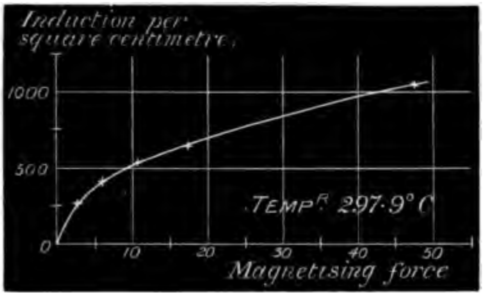
CURVE X.



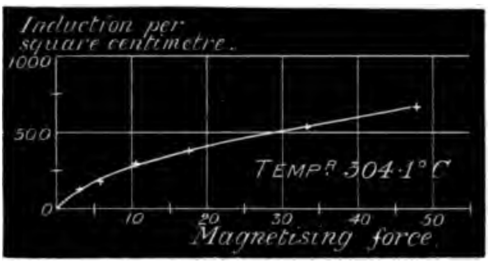

CURVE XI.



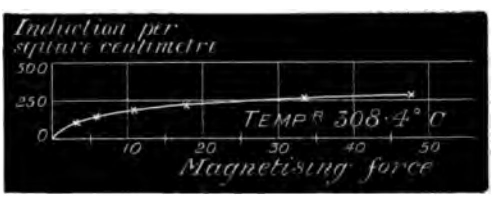
Curve XII.



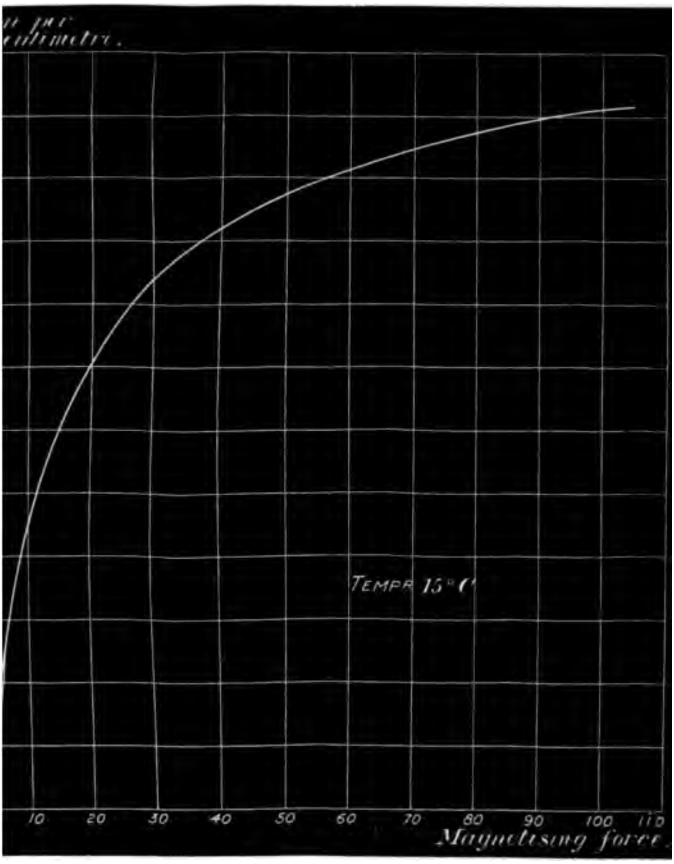

Curve XIII.



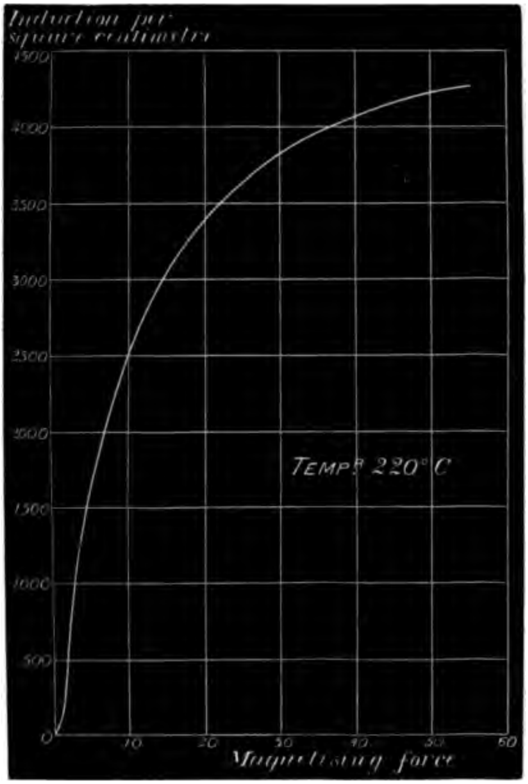

Curve XIV.



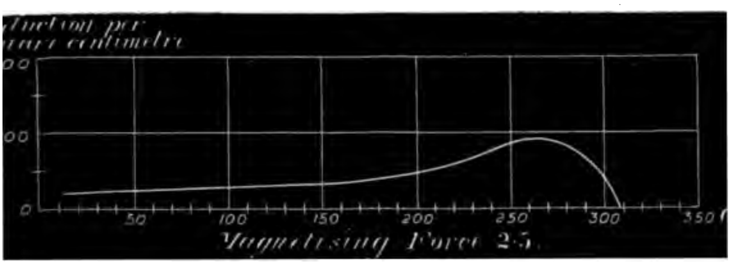
**Curve XV.**



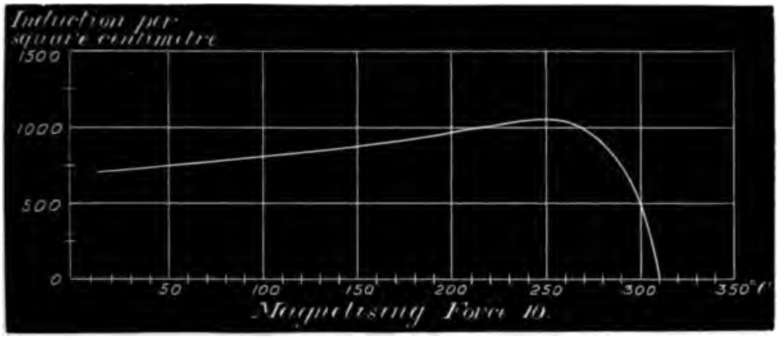
CURVE XVI.



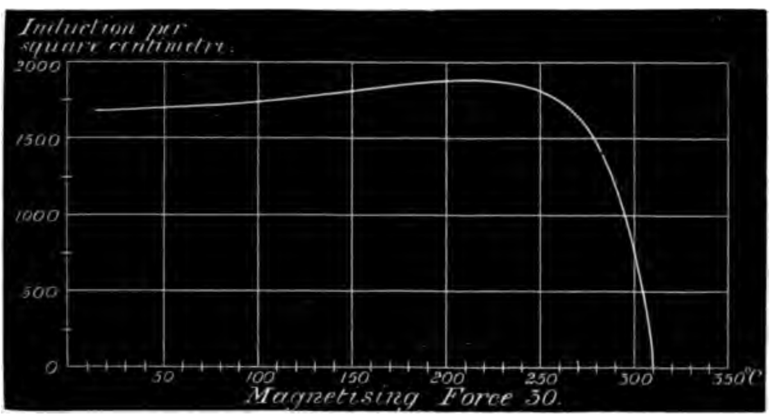

CURVE XVII.



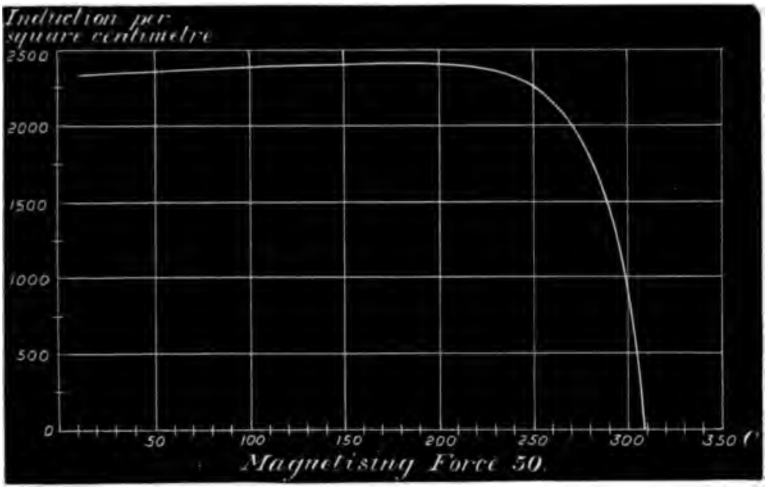
**Curve XVIII.**




**Curve XIX.**



CURVE XX.




CURVE XXI.

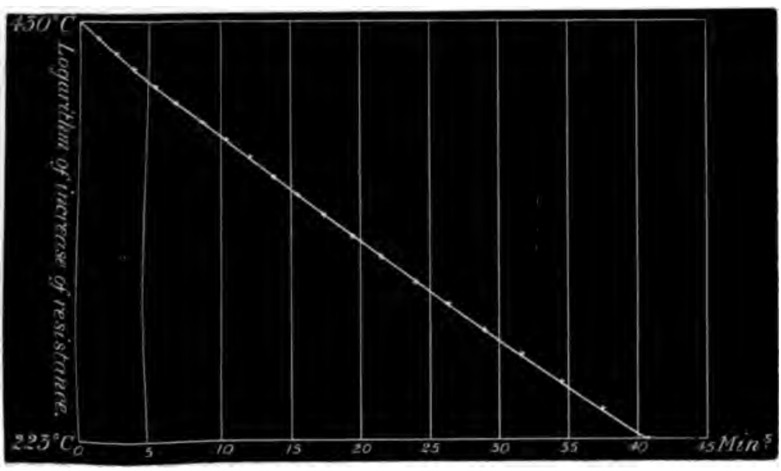
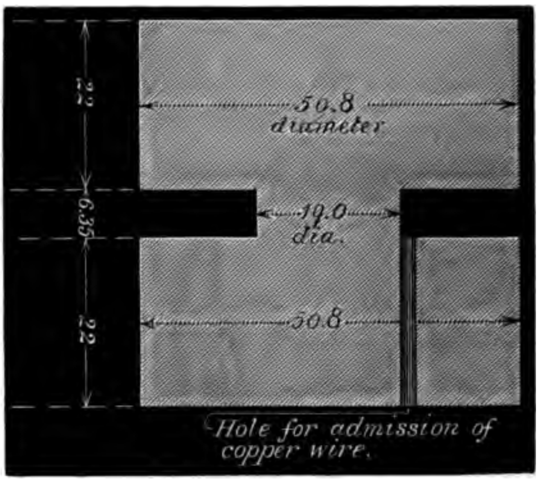


FIG. 2.


22
50.8
diameter
6.35
10.0
dia.
22
50.8
Hole for admission of
copper wire.

'he magnetisation of my impure nickel disappears about

. little below the temperature of 310° C. the induction shes very rapidly with increase of temperature.

ıt lower temperatures still the induction increases with rise of rature for low forces, diminishes for high forces. This fact has bserved by several experimenters.

:ific *Heat*.—The object here was simply to ascertain whether or ere was marked change at the temperature when the nickel to be magnetic. It appeared that this question could be best :ed by the method of cooling, and that it mattered little even 'ere roughly applied. A cylinder of nickel (fig. 2, Plate 13) ken, 5·08 cm. diameter, 5·08 cm. high, having a circumferential ·, 15·9 mm. deep and 6·35 mm. wide. In this groove was wound er wire, well insulated with asbestos, by the resistance of which mperature was determined. The cylinder was next enveloped in folds of asbestos paper to insure that the cooling should be slow, ıat consequently the temperature of the nickel should be fairly m and equal to that of the copper wire. The whole was now ı over a bunsen lamp till the temperature was considerably 310° C.; the lamp was next removed, and the times noted at the resistance of the copper wire was balanced by successive in the Wheatstone's bridge. If $\theta$ be the temperature, and $t$ be ınd if the specific heat be assumed constant, and the rate of heat proportional to the excess of temperature, $k\frac{d\theta}{dt}+\theta=0$ g $\theta+(t-t_0)=0$. In curve 21 the abscissæ represent the time in es, the ordinates the logarithms of the temperature; the points lie in a straight line if the specific heat were constant. It will ıerved that the curvature of the curve is small and regular, :ing that although the specific heat is not quite constant, or the : loss is not quite proportional to the excess of temperature, is no sudden change at or about 310° C. Hence we may infer ı this sample there is no great or sudden absorption or libera- ıf heat occurring with the accession of the property of :tisability.

VIII. "Experiments on Carbon at high Temperatures and under great Pressures, and in contact with other Substances." By the Hon. CHARLES A. PARSONS. Communicated by the Right Hon. the EARL OF ROSSE, F.R.S. Received June 13, 1888.

The primary object of these experiments was to obtain a dense form of carbon which should be more durable than the ordinary carbon when used in arc lamps, and at the same time to obtain a material better suited for the formation of the burners of incandescent lamps.

There were a considerable number of experiments made in which the conditions were somewhat alike, and many were almost repetitions with slightly varying pressures and temperatures. They may, however be divided into two distinct classes: the first in which a carbon rod surrounded by a fluid under great pressure is electrically heated by passing a large current through it, the second in which the liquid is replaced by various substances such as alumina, silica, lime, &c.

The arrangement of the experiment was as follows:—A massive cylindrical steel mould of about 3 inches internal diameter and 6 inches high was placed under a hydraulic press; the bottom of the mould was closed by a spigot and asbestos-rubber packing—similar to the gas-check in guns; the top was closed by a plunger similarly packed; this packing was perfectly tight at all pressures. In the spigot was a centrally bored hole into which the bottom end of the carbon rod to be treated fitted, the top end of the carbon rod was connected electrically to the mould by a copper cap which also helped to support the carbon rod in a central position. The bottom block and spigot were insulated electrically from the mould by asbestos, and the leading wires from the dynamo being connected to the block and mould respectively, the current passed along the carbon rod in the interior of the mould.

The fluid was run in so as to cover the rod completely. The plunger was then free to exert its pressure on the liquid without injuring the carbon. The pressure in the mould was indicated by the gauge on the press.

*Experiments. Class I.*

Among the liquids tested were benzene, paraffin, treacle, chloride and bisulphide of carbon.

The pressures in the mould during the several experiments were maintained at from 5 to 15 tons per square inch; the initial size of the rod was in all cases $\frac{1}{4}$-inch, and the current from 100 to 300 ampères.

*Results.*—In some of these experiments a considerable quantity of gas was generated, and the press had to be slightly slacked back during the experiment to accommodate it and maintain the pressure constant.

In all cases there was a soft friable black deposit of considerable thickness on the carbon.

In no case was the specific gravity of the carbon rod increased by this process. There was no change in appearance of the fracture, excepting when chloride of carbon had been the fluid; it was greyer in this case.

The rate of burning of samples placed in arc lamps was not diminished by the process. Various rates of deposition were tried, but with the same result, and the conclusion seems to be that under very high pressures, such as from 5 to 15 tons per square inch, the deposit of carbon by heat from hydrocarbons, chloride of carbon, bisulphide of carbon, treacle, &c., is of a sooty nature, and unlike the hard steel-grey deposit from the same liquids or their vapours at atmospheric or lower pressures.

*Experiments. Class II.*

In these experiments the asbestos-rubber packing was omitted, the plunger and spigot being an easy fit in the mould. A layer of coke powder under the plunger formed the top electrical connexion with the rod.

No. I. Silver sand or silica was run around the carbon rod, and pressures of from 5 to 30 tons per square inch applied; the rod was usually about ¼-inch diameter, and currents up to 300 ampères passed.

*Results.*—The silica was melted to the form of a small hen's egg around the rod. When the current was increased to about 250 ampères the rod became altered to graphite, the greater the heat apparently the softer the graphite. There was no action between the silica and the carbon, the surface of the carbon remained black, and there were no hard particles in or on the carbon rod.

Other substances, such as an hydrated alumina and mixtures of alumina and silica, gave the same results.

The density of the carbon was considerably increased, in some cases from normal at 1·6 to 2·2 and 2·4; in these cases the carbon appeared very dense, much harder than the original carbon, and about as hard as the densest gas-retort carbon. No crystalline structure was visible.

The specimens were treated with solvents, and there appeared no indication of the surrounding substance having penetrated the rod; the carbon was undoubtedly consolidated by 30 per cent.

In some cases when the material surrounding the rod was alumina

saturated with oil, soft crystals of graphite exuded from specimens that had been kept for some weeks.

No. 2. Pure hydrated alumina, carbonate and oxide of magnesia and lime all rapidly destroyed the carbon rod, by combining with it, the hydrated alumina forming large volumes of gas of which it appeared to be a constituent. On account of the great diminution of bulk, no analysis was made; the gas issued from the mould explosively at from 10 to 12 tons per square inch. The alumina was found in a crystalline crust, like sugar, around where the rod had been. Hardness that of corundum, almost translucent.

No. 3. The following is the most interesting experiment of the series:—

On the botton of the mould was a layer of slaked lime about $\frac{1}{4}$-inch thick, over this silver sand 2 inches, then another layer of lime of the same thickness as the former, finally a layer of coke-dust and then the plunger. With a pressure of from 5 to 30 tons per square inch in the mould, and the carbon of from $\frac{1}{4}$ to $\frac{5}{16}$ diameter, currents from 200 to 300 ampères were passed.

In from 10 to 30 minutes the current was generally interrupted by the breaking or fusing of the rod, or by the action of the lime in dissolving it at the top or bottom. On opening the mould when it had cooled a little, the silica usually appeared to have melted to an egg shaped mass, and mixed somewhat at the ends with the lime; the surface of the carbon appeared acted on, and sometimes pitted and crystalline in places; silica adhered to the surface, and beneath, when viewed under the microscope, appeared a globular cauliflower-like formation of a yellowish colour, resembling some specimens of "bort."*

After several days' immersion in concentrated hydrofluoric acid, this formation remained partly adherent to the carbon; on the surface of the carbon was a layer or skin about $\frac{1}{64}$th of an inch thick of great hardness, on the outside grey, the fracture greyer than the carbon but having a shining coke-like appearance under the microscope.

The powder scraped off the surface of the rod has great hardness and will cut rock crystal when applied with a piece of metal faster than emery powder. It has, under the microscope, the appearance of bort, the minute particles seem to cling together; they are not transparent as a rule, and though some such particles are found among them, it is not clear that such are hard.

When a piece of the skin has been rubbed against a diamond or other hard body, the projecting or hard portions have a glossy coke like appearance.

A piece of the skin will continue to scratch rock crystal for some time without losing its edge. It will scratch ruby, and when rubbed

* *The bort-like* powder is not acted on by hydrofluoric and nitric acids mixed.

for some time against it will wear grooves or facets upon it. When a cut diamond is rubbed on the surface of the skin, it will cut through it into the carbon beneath, making a black line or opening about $\frac{1}{4}$-inch long; the facet on the diamond, originally $\frac{1}{32}$-inch diameter, will have its corners evenly rounded, and its polished surface reduced to about one-half its original area; the appearance of the edges is as if they had been rubbed down by a nearly equally hard substance.

The subject of the last experiment is scarcely sufficiently investigated to warrant any definite conclusions.

The substance in the several ways it has so far been tested seems to possess a hardness of nearly if not quite the first quality. The minuteness of the particles, which appear more or less cemented together, and are less cohesive after the action of acid, make it very difficult to determine their distinctive features.

The mode of formation is not inconsistent with the conditions of pressure, temperature, and the presence of moisture, lime, silica, and other substances as they appear to have existed in the craters or spouts of the Cape Diamond Mines at some epoch.

From the few experiments that have been made it appears that at pressures below 3 tons per square inch, the deposit does not possess the same hardness, though somewhat similar in appearance.

What part the lime and silica play, whether the former only supplies moisture and oxygen which combine with the carbon, or whether the presence of lime is necessary to the action, is not clear.

We may, however, observe that so far it seems as if the lime and moisture combining with the carbon form a gas or liquid at great pressure, which combining with the silica, forms some compound of lime, silica, and carbon, or perhaps pure carbon only, of great hardness.

*Presents, June* 14, 1888.

Transactions.

Albany:—New York State Museum of Natural History. Bulletin. No. 3. 8vo. *Albany* 1888. The Museum.

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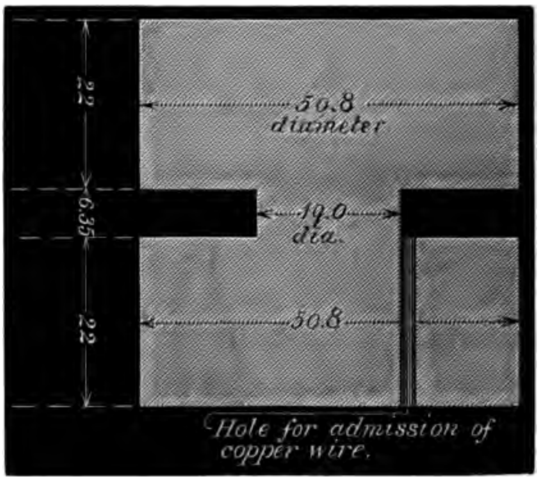
FIG. 2.


22
50.8
diameter
6.35
19.0
dia.
22
50.8
Hole for admission of
copper wire.

towards the aboral side, the stomach forms the well-known pentagonal "pyloric sac." The pyloric sac gives off five radial ducts, each of which divides into two tubules bearing a number of lateral follicles, whose secretions are poured into the pyloric sac and intestine. The author has proved the nature of their secretion to be similar to that of the pancreatic fluid of the Vertebrata ('Edinburgh, Roy. Soc. Proc.,' No. 125, p. 120). Recently, the secretion found in the five pouches of the stomach (of *Uraster*) has been submitted to a careful chemical and microscopical examination. With a quantity of the secretion, obtained from a large number of starfishes, the following experiments were performed:—

1. The clear liquid from these sacs was treated with a hot dilute solution of sodium hydrate. On the addition of pure hydrochloric acid, a slight flaky precipitate was obtained, after standing seven and a half hours. These flakes when examined beneath the microscope ($\frac{1}{6}$-in. obj.) were seen to consist of various crystalline forms, the predominant forms being those of the rhomb. On treating the secretion alone with alcohol rhombic crystals are deposited, which are soluble in water. When these crystals are treated with nitric acid and then gently heated with ammonia, reddish-purple murexide is obtained, crystallised in microscopic prisms.

2. Another method was used for testing the secretion. It (the secretion) was boiled in distilled water and evaporated carefully to dryness. The residue obtained was treated with absolute alcohol and filtered. Boiling water was poured upon the residue, and to the aqueous filtrate an excess of acetic acid was added. After standing some hours, crystals of *uric acid* were deposited and easily recognised by the chemico-microscopical tests mentioned above.

The above alcoholic filtrate was tested for *urea*. First of all, the alcoholic solution was diluted with distilled water, and boiled over a water-bath until all the alcohol had vaporised. The warm aqueous solution (A) remaining was now tested for urea, in the following manner:—

(*a*.) On the addition of a solution of mercuric nitrate to a portion of the above solution, *no* white precipitate was obtained.

(*b*.) To another portion of the solution (A), a solution of sodium hypochlorite was added. No bubbles of nitrogen were disengaged.

(*c*.) No crystals of urea nitrate were formed in a small quantity of the solution (A) [concentrated by evaporation] after the addition of nitric acid.

(*d*.) The distillation of a small quantity of the solution (A) with *pure* sodium carbonate, in a chemically clean Wurtz's flask attached to a small Liebig's condenser, failed to produce in the distillate any coloration with Nessler's reagent.

The above tests clearly prove the entire absence of urea in the secretion under examination. No guanin or calcium phosphate could be detected in the secretion, although the author has found the latter compound as an ingredient in the renal secretions of the Cephalopoda and the Lamellibranchiata ('Edinburgh, Roy. Soc. Proc.,' vol. 14, p. 230).

From this investigation, the isolation of uric acid proves the renal function of the five pouches of the stomach of the Asteridea.

II. *The Salivary Glands of* Sepia officinalis *and* Patella vulgata.

The author has already made a study of the nephridia and the so-called "livers" in both these forms of the Invertebrata (see the memoirs, *loc. cit.*). Since then he has studied the chemico-physiological reactions of the secretion produced by the salivary glands of the cuttle-fish and the limpet, these organisms representing two important orders of the Mollusca.

(1.) *Sepia officinalis.*

There are two pairs of salivary glands in *Sepia officinalis.* The posterior pair, which are the largest, lie on either side of the œsophagus. The secretion of the posterior glands is poured into the œsophagus, while the secretion of the smaller anterior pair of glands passes directly into the buccal cavity. A quantity of the secretion was extracted by using several freshly killed cuttle-fishes. It was alkaline to test-papers. A portion of the secretion was added to a small quantity of starch, the starch being converted into glucose sugar in 15 minutes. The presence of glucose was proved by the formation of red cuprous oxide by the action of Fehling's solution. The soluble zymase (ferment) contained in the secretion (which is capable of causing the hydration of starch), was isolated by precipitating the secretion with dilute normal phosphoric acid, adding lime-water and then filtering. The precipitate produced was dissolved in distilled water and reprecipitated by alcohol. This precipitate converts starch into glucose sugar.

When a drop of the clear secretion is allowed to fall into a beaker containing dilute acetic acid, stringy flakes of *mucin* are easily obtained. The presence of mucin was confirmed by several well-known tests.

Another portion of the secretion was distilled (with the utmost care) with dilute sulphuric acid, and to the distillate ferric chloride solution was added, which gave a red colour, indicating the presence *of sulphocyanates.*

*The inorganic constituent*, as far as the author could make out,

consists only of phosphate of calcium. No calcium carbonate could be detected.

There is much in favour of the supposition that the *diastatic ferment* in these secretions is produced as the result of the action of nerve-fibres (from the inferior buccal ganglion) upon the protoplasm of the epithelium cells of the glands.

The author intends to examine various organs in other genera and species of the Decapoda, especially those inhabiting the Japanese seas.

### (2.) *Patella vulgata.*

The two salivary glands of Patella are well-marked and situated anteriorly to the pharynx, lying beneath the pericardium on one side and the renal and anal papillæ on the other. They are of a yellowish-brown colour and give off four ducts. The secretion of these glands was examined by the same method applied to the salivary glands of *Sepia officinalis*, and with similar results.

The following table represents the constituents found in the salivary secretions of the two orders of the Mollusca already investigated:—

| | Cephalopoda. | Gasteropoda. | |
|---|---|---|---|
| | (*a.*) Dibranchiata. | (*a.*) Pulmogasteropoda.* | (*b.*) Branchiogasteropoda. |
| Soluble diastatic ferment | present | present | present |
| Mucin | present | .. | present |
| Sulphocyanates | present | ? | present |
| Calcium phosphate | present | ? | present |

Investigations indicate that the salivary glands of the Cephalopoda and Gasteropoda are similar in physiological function to the salivary glands of the Vertebrata.

* 'Edinburgh, Roy. Soc. Proc.,' vol. 14, p. 236.

II. "Muscular Movements in Man, and their Evolution in the Infant: a Study of Movement in Man, and its Evolution, together with Inferences as to the Properties of Nerve-centres and their Modes of Action in expressing Thought." By Francis Warner, M.D., F.R.C.P., Physician to the London Hospital and Lecturer on Botany in the London Hospital Medical College. Communicated by Professor J. Hutchinson, F.R.S. Received June 12, 1888.

(Abstract.)

Movements as signs of brain action have long been studied by the physiologist; but before proceeding to give an account of the visible evolution of voluntary movement in man, it is necessary to define the different classes of movements seen, indicating the criteria by which the observer may be guided in the examples before him. Movements may be classed according to the parts moving, the time, and the quantity of each movement. These are the only intrinsic attributes of such acts. If the nerve-centres which send stimuli to the muscles are acting *in equilibrio*, the static outcome is seen in the postures resulting in the body; hence postures are signs of the ratios of action in the nerve-centres, and indicate their present state or mode of action. Typical postures and movements are described. A variation in the ratios of action in the centres leads to visible movement. Certain postures and movements are found by experience to correspond to certain recognised brain states. Movements may occur in combinations and in series; special combinations and series of movements determine the outcome of the action of which they are component parts. It is shown that the time of action in the various centres thus determines the outcome of the action, and is itself controlled by impressions received through the senses. When movements are seen, not controlled by present circumstances, they are probably the result of antecedent or inherited impressions; such are called spontaneous.

*Section II. Evolution of Movements in Man.*

The new-born infant presents constant movement in all its parts while it is awake, and this is not controlled by impressions from without. Graphic tracings of such movements are given. This spontaneous movement in the infant appears to be of great physiological importance, and is here termed "microkinesis." It is argued that the mode of brain action which produces microkinesis is analogous to the action producing spontaneous movements in all young animals, and to the modes of cell-growth which produce circumnutation in

young seedling plants. It is argued that as circumnutation becomes modified by external forces to the modes of movement termed heliotropism, geotropism, &c., so microkinesis in the infant is replaced by the more complicated modes of brain action as evolution proceeds.

The conditions of movement are then described, as seen at successive stages of development of the child, and it is shown that they become less spontaneous, and more under control of stimuli acting upon the child from without, while the phenomena termed memory and imitation are evolved.

*Section III. Properties of Nerve-centres and their Modes of Action.*

From observations made, descriptions are given of the modes of action and properties of nerve-centres in adult age, such descriptions being given in terms implying visible movements. Impressionability, imitation, and retentiveness are thus described. Nerve-centres are said to be "free" when only slightly stimulated. Delayed expression of impressions are seen when the visible outcome is delayed after the stimulus which produced it. Double-action is said to occur when a local effect and a distant one, occur from one impression. Compound cerebral action is said to occur, when the study of the visible movements indicates that successive unions of centres are in action, leading to a visible outcome well adapted to the primary stimulus which produced the series. When a slight stimulus leads to a spreading area of movements producing considerable force, the phenomenon is termed reinforcement.

From observations made, two hypotheses are put forward. It is suggested that when a well co-ordinated movement follows a slight stimulus, the impression produces temporary unions among the centres, preparing them for the special combinations and series of actions which are seen to follow. Such unions among nerve-centres appear to be formed when a period of cerebral inhibition, produced by a word of command, is seen to be followed by a co-ordinated series of acts. A graphic tracing indicating suspension of microkinesis to the stimulus of sight and sound is given. It is further suggested that the brain action corresponding to thought, is the formation of functional unions among cells, whose outcome is seen in the movements which express the thought, or its physical representation. Properties similar to those described in brain centres may be illustrated in modes of growth. Intelligence is then not a property of the brain, *per se*, but for its manifestation certain modes of brain action are necessary. In the special postures and movements described, a number of physical signs of brain states are offered to the clinical observer.

## III. "On the Electromotive Changes connected with the Beat of the Mammalian Heart, and of the Human Heart in particular." By AUGUSTUS D. WALLER, M.D. Communicated by Professor BURDON SANDERSON, F.R.S. Received June 12, 1888.

(Abstract.)

1. Description of experiments in which the electrical variation connected with the spontaneous beat is modified.
2. The normal ventricular variation is diphasic, and usually indicates (1) negativity of apex, (2) negativity of base.
3. Description of "irregular" variations.
4. Observations on animals with one or both leading off electrodes applied to the body at a distance from the heart.
5. Determination of the electrical variations of the heart on man.
6. The variation is diphasic, and indicates (1) negativity of apex, (2) negativity of base.
7. Distribution of cardiac potential in man and animals. "Favourable" and "unfavourable" combinations.
8. Demonstration of electrical effects by leading off from the surface of the intact body by the various extremities and natural orifices.
9. Comparison between effects observed on man with the normal and with a transposed situation of the viscera.

## IV. "On the Plasticity* of Glacier and other Ice." By JAMES C. MCCONNEL, M.A., Fellow of Clare College, Cambridge, and DUDLEY A. KIDD. Communicated by R. T. GLAZEBROOK, F.R.S. Received June 11, 1888.

The experiments described in the following paper were undertaken in continuation of those made by Dr. Main in the winter 1886–87, and described by him in a paper† read before the Royal Society the following summer. The investigation is by no means complete, but the results hitherto obtained seem to us sufficiently novel and important to be worthy of being put on record, while we hope to

* Dr. Main used the term "viscosity." But this has been always applied in liquids to molecular friction, and we have the authority of Sir Wm. Thomson ('Encycl. Britann.,' Art.: *Elasticity*, p. 7) for reserving it for the same property in solids also, leaving "plasticity" to denote continuous yielding under stress.

† 'Roy. Soc. Proc.,' vol. 42, p. 329.

prosecute the subject further next winter. We shall first give a general account of our results, and then describe the experiments in more full detail.

Main found that a bar of ice, which had been formed in a mould,* yielded slowly but continuously to tension, though kept at a temperature some degrees below freezing point. We began work under the impression that the rate of extension depended mainly on the temperature and tension, and that the chief difficulty lay in keeping the temperature constant. But by a happy chance our very first experiment showed us that not merely the rate, but even the very existence of the extension depended on the structure of the ice. And this is a matter which seems to have been quite disregarded by previous experimenters.†

After many, and for the most part unsuccessful, attempts to obtain a piece of perfectly clear ice, frozen in the mould used by Main, we took a bar cut from the clear ice formed on the surface of a bath of water, and froze its ends on to blocks of ice fitting the two conical collars through which the tension is applied. To avoid any question as to the ice giving way in the collars, where it is subjected to pressure as well as tension—the bar was pierced near either end by a steel needle firmly frozen in, and the measurements were taken between the projecting ends of these needles. We found to our astonishment that the stretching was almost *nil*, though the tension was decidedly greater than that usually applied by Main. There was a slight extension at first, but during the last five days the extension observed was at the mean rate of only 0·00031 mm. per hour per length of 10 cm., and this may well be attributed to the rise of temperature which took place. The rigidity cannot have been due to the cold, for during the last 24 hours the temperature was between $-1°$ and $-2°$.‡ After the experiment, the ice was examined under the polariscope, and found to be a single regular crystal showing the coloured rings and black cross very well. The optic axis was at right angles to the length of the bar. This experiment showed it was a very necessary precaution to take the measurements between needles fixed in the bar itself. For whether the bar extended or not, the movement of the index H (fig. 2), showed

* The mould produced a round bar of ice 24 cm. in length and 2·8 cm. in diameter, with a conical expansion at the lower end to fit into an iron collar C (fig. 2), through which the tension could be applied. The other end of the bar was frozen on to ice filling a similar collar B. These iron collars were faced with carefully worked brass plates, and Main determined the extension by measuring the distance between the plates with callipers.—July 6, 1888.

† *See Heim,* 'Handbuch der Gletscherkunde,' published by Engelhorn, Stuttgart, *1885, p. 315.*

‡ *We use* the centigrade scale of temperature throughout.

a decided separation of the collars due to the plasticity of the conical pieces of ice therein.

We next took a bar of ice formed in the mould, applied tension and took measurements in the same way. The extension was at the rate of 0·048 mm. per hour per length of 10 cm. The crystalline structure of this ice was highly irregular. As one principal object of our experiments lay in their application to the theory of glaciers, it had now become obviously most important to test actual glacier ice. We therefore drove over to the Morteratsch glacier, which is now readily accessible from St. Moritz even in the winter, and obtained some specimens from the natural ice caves at the foot of the glacier.

We tested three pieces, which were quite sufficient to disprove the common notions, that glacier ice is only plastic under pressure, not under tension, and that regelation is an essential part of the process. They showed at the same time the extraordinary variability of the phenomenon. The first extended at a rate of from 0·013 mm. to 0·022 mm. per hour per length of 10 cm., the variations in speed being attributable to temperature. The second piece began at a rate of 0·016 mm. and gradually slowed down till it reached at the same temperature a rate of 0·0029 mm., at which point it remained tolerably constant, except for temperature variations, till a greater tension was applied. The third piece on the contrary began at the rate of 0·012 mm., increased its speed with greater tension to 0·026 mm., and stretched faster and faster with unaltered tension, till it reached the extraordinary speed of 1·88 mm. per hour per length of 10 cm. We put on a check by reducing the tension slightly, whereupon the speed fell at once to 0·35 mm. and gradually declined to 0·043 mm. The lowest temperature reached during our experiments, except with the intractable bath ice, was with this specimen. During 12 hours with a maximum temperature $-9°$ and a mean temperature probably $-10·5°$, the rate under the light tension of 1·45 kilo. per sq. cm. was 0·0065 mm.

These three pieces were composed of a number of crystals varying in thickness from two or three millimetres up to thirty or even a hundred. These crystals are the "glacier grains" (*gletscherkörner*), which play such a large part in glacier literature. Glacier ice is a sort of conglomerate of these grains, differing, however, from a conglomerate proper in that there is no matrix, the grains fitting each other perfectly. In the winter, at any rate, the ice on the sides of the glacier caves looks quite homogeneous. But, when a piece is broken off and exposed to the sun's rays, the different grains become visible to the naked eye, being separated probably by thin films of water. Though the optical structure of each grain is found under the polariscope to be perfectly uniform, the bounding surfaces are utterly irregular, and are generally curved. The optic axes too of

neighbouring grains seem to be arranged quite at random. Owing to the structure being so complex, we failed to trace any relation between the arrangement of the crystals and the rapidity of extension. It is true that the most rigid piece of the three was composed of small crystals, while the most plastic contained one very large crystal; but this was perhaps accidental. Fortunately, we were able to obtain ice of a more regular structure, which has already thrown a little light on the action at the interfaces of the crystals, and offers an attractive field to further investigation.

Some of the ice of the St. Moritz lake is built up of vertical columns,* from a centimetre downwards in diameter, and in length equal to the thickness of the clear ice, *i.e.*, a foot or more. A horizontal section, exposed to the sun for a few minutes, shows the irregular mosaic pattern of the divisions between the columns. The thickness of each column is not perfectly uniform. Sometimes indeed one thins out to a sharp point at the lower end. Each column is a single crystal, and the optic axes are generally nearly horizontal. Some experiments on freezing water in a bath, lead us to attribute this curious structure to the first layer of ice having been formed rapidly, in air, for instance, below $-6°$ C. We found that if the first layer had been formed slowly, and was therefore homogeneous with the axis vertical, a very cold night would only increase the thickness of the ice, while maintaining its regularity.

We applied tension to a bar of lake ice carefully cut parallel to the columns. It stretched indeed, but excessively slowly. During seven days it stretched at the rate of only 0·0004 mm. per hour per length of 10 cm., though at one time the temperature of the surrounding air went up above zero. The tension was 2 kilos. per sq. cm. This slight extension may well be attributed to the tension not being exactly parallel to the interfaces of the columns. This experiment corroborates our first result, that a single crystal will not stretch at right angles to its optic axis. We next cut a bar at about 45° to the length of the columns, and the difference was very manifest. During 80 hours under a tension of 2·75 kilos. per sq. cm., it extended at the rate of 0·015 mm. per hour per length of 10 cm., nearly 40 times as fast.

An icicle is an example of ice formed of very minute crystals irregularly arranged. We found that an icicle under a tension of 2·2 kilos. per sq. cm. stretched at the rate of 0·003 mm. per hour per length of 10 cm. This is very slow, especially as the temperature

* This was the case in all pieces obtained from one end of the lake, where men were cutting ice for storage purposes, whether new ice or old. In a part, however, which had frozen a few days earlier, further out from the shore, we found much larger crystals with the axes nearly vertical but not quite parallel to each other.—*July 6, 1888.*

was high, averaging $-1°$ C., yet it is difficult to suggest any theoretical reason for an increase in the number of interfaces producing a decrease in the plasticity.

We tried further two experiments on compression of ice, the pressure being applied to three nearly cubical pieces at once. Of three pieces of glacier ice, under a pressure of 3·2 kilos. per sq. cm., the mean rates of contraction during five days were respectively 0·035 mm., 0·056 mm., and 0·007 mm. per hour per length of 10 cm. These figures show that while the plasticity varies enormously in different specimens, the rate of distortion is of the same order of magnitude, whether the force applied be a pull or a thrust.

The other experiment was on three pieces of lake ice, applying the pressure in a direction parallel to the columns. The contraction was scarcely perceptible. Under a pressure of 3·7 kilos. per sq. cm., the mean rate of the three pieces during four days was 0·001 mm. per hour per length of 10 cm. To fix the blocks of ice in position, we found it necessary to cover their ends with paper frozen on, and the small contraction observed may well be attributed to the yielding of the films of irregular ice with which the paper was attached. This view is supported by the fact that nearly the whole of the contraction took place in the first 36 hours.

We have now shown by direct experiment that ordinary ice, consisting of an irregular aggregation of crystals, exhibits plasticity, both under pressure and under tension, at temperatures far below the freezing point—in the case of tension at any rate down to $-9°$ at least, and probably much lower—and also that a single uniform crystal will not yield continuously either to pressure or tension when applied in a direction at right angles to the optic axis. We fully intended to test a crystal under tension applied along the optic axis; but we were unsuccessful in obtaining a crystal longer in the axis than perhaps 8 cm., and when we had decided to be content with that length, a thaw put a stop to all further operations. We have, however, very little doubt that a crystal would refuse to yield either to pressure or to tension in whatever direction they were applied.

The following reasoning seems tolerably conclusive as far as it goes. We first assume the axiom that, if two systems of stresses produce each by itself no continuous yielding, superposition of the two will likewise produce no continuous yielding. This will probably be admitted when we add the proviso that, when the nature of the resultant stresses is found, their magnitude is to be reduced to the same value as that of the simple stresses which are known to be inactive. Take then a cube of ice, two of whose faces are perpendicular *to the optic axis.* Apply tension to one of the other pairs of *faces. This according to* our experiments produces no extension

Of course we do not take into account the slight elastic yielding. Apply an equal tension to the other pair of faces which are paralle to the axis. There is still no extension by the axiom. Now it ca hardly be supposed that an uniform hydrostatic pressure could pro duce continuous change of form. Apply then a pressure of suc magnitude as to neutralise the two tensions. We have then remain ing only a pressure along the optic axis, producing no continuou yielding.

In a similar way it may be shown that tension along the optic ax would produce no continuous yielding. It is true that the reasonin cannot be extended to pressures and tensions oblique to the optic axi But if the plasticity observed had been due to the majority of crysta extending, while a certain number remained unchanged, there woul surely have been numerous cracks found in every case; while as matter of fact such cracks were only found in two cases, and the they were very slight. Hence, while we think it desirable to exper ment further in the matter, we feel tolerably confident that singl crystals of ice are not plastic, and we attribute the apparent plasticit of glacier ice to some action at the interfaces of the crystals. Bu we are not at present inclined to venture any opinion as to the natu of this action.

The variation of plasticity with the temperature is of great intere both for the theory of glaciers and for the explanation of the plasticit itself, but it is so difficult to disentangle the temperature variatio proper from the much larger alterations due to structural change that our experiments throw very little light on this point. In the ca of the glacier ice in Experiment 7 the rate seems to have becon tolerably constant except for temperature changes. While at $-3 \cdot 5°$ th rate was 0·0029, two days before and two days afterwards it wa about 0·0020 at $-5°$, and a few days earlier 0·0013 at $-8°$. In th icicle, when the temperature variations seemed paramount, the rate a $-2°$ was 0·0028, and at $-0 \cdot 2°$ 0·0034. This is a much smaller chang than we should have expected. In the case of compression the influ ence of temperature seems more strongly marked. In all thre pieces the rate rose at $-3°$ to about ten times its value at $-5°$. A increase which takes place in three pieces simultaneously can hardl be attributed to structural changes independent of the temperature.

The change in the rate of extension, produced by an alteration o the tension, was in every case altogether out of proportion to th magnitude of the latter. In the following table are collected all th instances which occurred:—

| Specimen. | Change of tension. | Change of rate. |
|---|---|---|
| | kg. per sq. cm. | mm. per hour per 10 cm. |
| Glacier ice C...... | 2·55 to 3·85 | 0·0018 to 0·0110 |
| Glacier ice D...... | 1·45 „ 2·55 | 0·0075 „ 0·026 |
| „ ...... | 2·55 „ 1·03 | 0·105? „ 0·010 |
| „ ...... | 1·03 „ 2·50 | 0·010 „ 0·228 |
| „ ...... | 2·50 „ 1·80 | 1·88 „ 0·35 |

The changes of temperature in these cases were insignificant com- red with the alteration of rate. The 0·105 is uncertain owing to an cident. It was certainly not less, and may have been a good deal eater.

We append a summary of some of our results arranged in tabular rm. Glacier ice C was the same piece as B, cut rather shorter.

[redacted]



Summary. Extension Experiments.

| Experiment No. | Description of specimen. | Duration. | Rate per hour in mm. per length of 10 cm. | Tension kilos. per sq. cm. | Maximum temperature. | Mean temperature. |
|---|---|---|---|---|---|---|
| 2 | Bath ice, uncorrected for temperature | 5½ days | 0·00028 | 4·9 | −1·0° | − 4·5° |
| " | " corrected for temperature | " | 0·00000 | " | " | " |
| 3 | Mould ice | 28 hours | 0·048 | 3·8 | 0·0 | − 5·0? |
| 4 | Glacier ice A, maximum rate | 5 " | 0·022 | 1·66 | 0·0 | − 2·0 |
| " | " minimum rate | 4 " | 0·013 | " | −1·0 | − 2·5 |
| 5 | Glacier ice B, maximum rate | 24 " | 0·016 | 2·7 | −2·5 | − 3·5 |
| 7 | Glacier ice C, " | 23 " | 0·0068 | 2·55 | −2·5 | − 4·5 |
| " | " minimum rate | 3 days | 0·0013 | " | −6·0 | − 9·0 |
| 8 | Glacier ice D, maximum rate | 10 mins. | 1·88 | 2·50 | −2·1 | − 2·1 |
| " | " minimum rate | 16 hours | 0·0054 | 1·45 | −6·0 | −10·0 |
| " | " lowest temperature | 12 " | 0·0065 | " | −9·0 | −10·5 |
| 6 | Icicle, maximum rate | 5 " | 0·0041 | 2·2 | 0·0 | 0·0 |
| " | " minimum rate | 8 " | 0·0015 | " | −0·7 | − 1·7 |
| 9 | Lake ice, parallel to columns | 7 days | 0·00039 | 2·1 | 0·0 | − 5·5 |
| " | " greater tension | 2 " | 0·00076 | 2·8 | −4·0 | − 5·5 |
| 10 | Lake ice, oblique to columns, maximum rate | 6 hours | 0·034 | 2·75 | −5·6 | − 5·8 |
| " | Lake ice, oblique to columns, minimum rate | 16 " | 0·010 | " | " | − 6·0 |

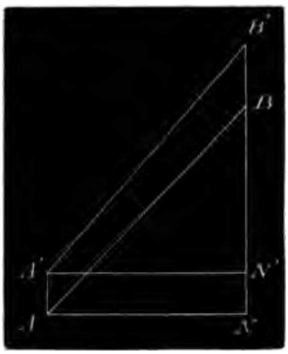
Compression Experiments.

| Experiment No. | Description of specimen. | Duration. | Rate per hour in mm. per length of 10 cm. | Pressure kilos. per sq. cm. | Maximum temperature. | Mean temperature. |
|---|---|---|---|---|---|---|
| 1 | Glacier ice E | 5 days | 0·035 | 3·2 | −2·8 | − 6·0 |
| " | Glacier ice F | " | 0·056 | " | " | " |
| " | Glacier ice G | " | 0·007 | " | " | " |
| 2 | Lake ice, parallel to columns A | 3 days | 0·0002 | 3·7 | −3·9 | − 6·0 |
| " | Lake ice, parallel to columns B | " | 0·0012 | " | " | " |
| " | Lake ice, parallel to columns C | " | 0·0018 | " | " | " |

'ill be interesting to make some numerical comparison between ures we have given and the plasticity actually observed in the ı of glaciers. Perhaps the most striking proof of the existence sticity is the great increase of velocity from the side to the of a glacier. A number of measurements on this point have ollected by Heim ('Gletscherkunde,' p. 147). The most rapid ɪe he mentions among the glaciers of the Alps is on the Rhone ', on a line 2300 metres above the top of the icefall. At etres from the western bank the mean yearly motion, 1874 to was 12·9 metres; at 160 metres from the bank it was metres. This gives an increase of velocity in each metre the glacier of 0·000058 metre per hour.

us consider what rate of extension this involves.

FIG. 1.




AB (fig. 1) be two points on a glacier moving in parallel direc- ɔf which B is moving faster. In the small time $\delta t$ (whose we may neglect) let A move to A' and B to B'. Draw AN, A'N' dicular to B'B produced. Let AN = A'N' = $a$, BN = $x$, B'N' ιB = $r$, A'B' = $r'$, and let the velocities be $v_a$ and $v_b$.

ı $$A'A = v_a\,\delta t, \qquad B'B = v_b\,\delta t,$$

$$r^2 = a^2+x^2,$$

$$r'^2 = a^2+x'^2 = a^2+\{x+(v_b-v_a)\delta t\}^2$$

$$= a^2+x^2+2x\,(v_b-v_a)\,\delta t$$

$$\therefore r'^2 = r^2\left(1+\frac{2x(v_b-v_a)\delta t}{a^2+x^2}\right),$$

and
$$r' = r\left(1+\frac{x(v_b-v_a)\delta t}{a^2+x^2}\right);$$

so
$$\frac{r'-r}{r} = \frac{x}{a^2+x^2}(v_b-v_a)\,\delta t. \quad \ldots\ldots\ldots\ldots$$

The expression $\frac{x}{a^2+x^2}$ is a maximum when $x = a$, and then we l
by (1)—

$$\frac{1}{r}\cdot\frac{r'-r}{\delta t} = \frac{v_b-v_a}{2a}. \quad \ldots\ldots\ldots\ldots$$

When $\delta t$ is very small, the ratio of $r'-r$ to $\delta t$ is the rate of incr of the distance between A and B. So, if we take any two point the glacier at unit distance, the rate of increase of the dist between them will be greatest when the line joining them is a to the direction of motion, and this maximum value will be equ one half the difference of the velocities of two points situated ab of each other and also at unit distance.

Thus the maximum rate of extension in the case we have take the Rhone glacier is 0·0029 mm. per hour per length of 10 cm. be it remembered, is the most rapid extension selected from a l number of measurements on different glaciers and at different ti and yet only one of the three specimens of glacier ice showed a less than this, and that was under one-third of the breaking ten The larger the specimen, the greater average plasticity would it play; for the addition of a small piece like our second specimen instance, would suffice to make a long rigid bar appear very pl Hence the glacier itself would be far more plastic than most s specimens taken at random from its mass. It would seem, there that neither the presence of crevasses nor a thawing temperatur essential conditions of the motion of a glacier. But that crev are found is not surprising, when we consider the rotten state of ice during the summer and the certainty that a crack, however sm once formed will continue as long as the tension exists. We be further that the stresses produced in a glacier by its own weigh comparable with those employed in our experiments.

## *Description of Apparatus.*

We had two sets of apparatus in operation. The first was employed by Dr. Main, figured and fully described in his pa We reproduce his figure unaltered, though we made a few alterat in the surrounding boxes. As we expected at first that our difficulty would be keeping the temperature constant, we made sp arrangements for overcoming this. To secure a large heat capac

introduced two tins, filled with a strong solution of salt, into the inner box, slightly altering its shape and increasing its size for this purpose. A broad shallow tin occupied the spare space at the top, and a tall tin occupied all the available space by the side of the ice between A and L (fig. 2). The space between the two boxes was filled with

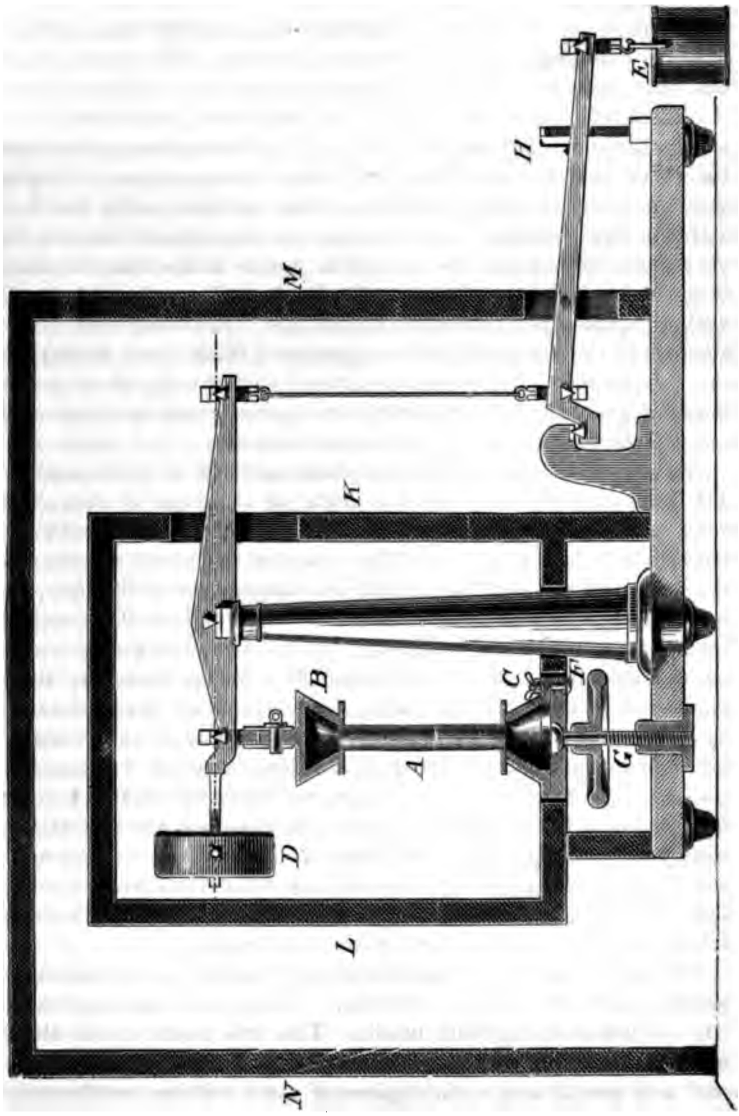



FIG. 2.

wood shavings, except between K and M. Here a wooden partition P was inserted to the left of the vertical connecting rod. The space between K and P was filled with wood shavings. To allow the lever to move freely, it passed through a wooden tube loosely packed with cotton-wool. The outer space between P and M was made fairly air-tight, and the opening through which the lower lever emerged was also plugged with cotton-wool.

The capacity of the inner chamber was about 60 litres, while the two tins contained about 25 litres of solution. The inner chamber was thus jacketed on all sides with a layer, from $10\frac{1}{2}$ to 20 cm. in thickness, of which from 4 to 6 cm. was solid wood and the rest wood shavings. To secure uniformity* of temperature the back of the inner chamber was lined with thick sheet-copper. Originally the front was similarly provided, a small aperture being cut for the cathetometer readings. But after the first experiment this was found very inconvenient and was discarded. Access to the box was obtained from the front, the space between the doors of the two boxes being filled with a movable pad stuffed with shavings. The inner door occupied about half the front of the ice-chamber. With these arrangements the temperature of the interior altered very slowly, often not more than a degreee in 24 hours, though no special precautions were taken to keep the temperature of the room constant.

We were not so successful in maintaining uniformity of temperature. The minimum thermometer was hung at the back of the chamber on a level with the middle of the ice. The maximum was placed with its bulb at the bottom of the chamber at the end removed from the tin. And we often found that the temperature at the time, shown by the maximum thermometer, was one or one and a half degrees lower than that shown by the minimum. In the temperatures given in the tables allowance is made for this. We found, however, that the variations in the plasticity due to the temperature were far exceeded by others, due probably to changes in the crystalline structure of the ice. In explanation of the considerable variation of temperature occasionally recorded in the tables, we must add that, in order to raise or lower the temperature, the inner chamber was sometimes left wholly or partially open. The front of the box was close by an open window, and was generally exposed to a decidedly lower temperature than the back, so that opening the doors to take the readings would seldom raise the internal temperature materially.

The bar of ice for an experiment was roughly sawn out and then shaped more carefully with a knife. A hole was bored near each end with a hot steel knitting needle. This was found to be the only method of making a hole free from the risk of splitting the ice. In each hole was frozen a short piece of steel knitting needle with the

* Uniformity refers to space, constancy to time.

ends projecting slightly. In the later experiments we used pieces of glass tube or rod for needles, to obviate any possible exaggeration of the extension through the needle bending in the ice. The glass had the further advantage of being a bad conductor of heat. We found that, when air above freezing point entered the chamber during the taking of a reading, the steel needles were apt to work loose, although the body of the ice had not time to materially rise in temperature. Such readings are of course discarded. The two conical collars were filled with ice by freezing water therein. The upper collar was taken out and inverted, and its brass plate levelled. Then the bar was carefully placed in a vertical position and frozen on. The bar was next hung in position in the chamber and frozen on to the ice on the lower collar *in situ*.

In the first experiment the measurement of the distance between the upper and lower needles was made with a cathetometer. On the two ends of each needle were glued pieces of paper, on each of which fine ink cross lines had been drawn. The cathetometer was not of the ordinary construction and merits a short description, as, though in practice it was not very successful, in principle it has, we believe, several advantages over the ordinary form. The stand consists of a vertical rod supported by three levelling screws. On this rod slides a metal block, provided with a clamp and slow-motion screw. The telescope rests on this block, being movable through ninety degrees about a vertical axis. The bearing of the telescope is the only mechanical part of the instrument that requires special care. For the cross wires of the ordinary telescope is substituted a micrometer scale. The millimetre scale is fixed on a separate stand as near as possible to the bar of ice and at the same distance from the telescope as the ice is, and is left untouched during the observations, so that it is of no consequence, for measuring small extensions, if it be not quite parallel to the direction of the tension. The distance from the telescope to the ice or to the scale was about 30 cm. On the top of the telescope is fixed a level. We carefully adjusted this, so that when the bubble was at its zero the axis of rotation of the telescope was in the vertical plane at right angles to the tube of spirit. Then if the bubble remained in its central position in every azimuth of the telescope, we could be sure the axis of rotation was vertical.

The observation was taken by reading the position, on the micrometer scale, of the image of the mark on the needle, then swinging the telescope round and reading the position, on the micrometer scale, of the two nearest divisions of the millimetre scale. By interpolation the exact height, on the millimetre scale, of the mark on the needle was then readily found. It will be noticed that the cathetometer need only *remain steady while* the telescope is swung round from the *needle to the scale;* whereas in the ordinary form there is a danger

of the whole stand being slightly displaced when the telescope is down to its lower position. In fact in our circumstances an ordi cathetometer would have been practically useless, owing to the ben of the floor and table at the slightest movement of the obse Observations, even with our special form, required the utmost The micrometer scale had twenty divisions, each 0·12 mm. in ac size, and corresponding to about 0·3 mm. on the other scale. magnification of the telescope, as compared with the eye at 9 in was about 5. This was scarcely great enough. We intended al have the micrometer divisions half the size, but the maker was able to graduate it so finely. Indeed, as it was, the lines were ra too thick.

By estimating tenths of the micrometer divisions we could rea 0·03 mm., but the readings might easily be at least 0·06 mm. in e Each determination of the length between the needles depends on readings, the upper needle, and its corresponding scale division, and lower needle, and its scale division. If the four readings happ to have each the maximum error 0·06 mm. with a suitable sign total error might be 0·24 mm. Such a combination of chanc highly improbable, but an error of 0·1 mm. is obviously not unli The cathetometer would have been a useful instrument for measu a large and regular extension with accuracy, but it was not ada to detect very small extensions, and a system of levers, whic adopted as a rough mode of measurement in our second se apparatus, proved so much more satisfactory and suitable to purposes, that we almost entirely discarded the cathetometer. contrivance is shown in fig. 3, in the form finally adopted. *a* a are sections of the projecting ends of glass needles fixed in the *cdef* is a bent iron wire, "the indicator," hooked to a wire loo securely fastened to *a*, *h* is a wooden lever suspended by a thre which owing to the counterpoise *k*, pulls the indicator upwards w thread fastened to a wire loop at *e*. The indicator is kept from ri by the connecting fibre, a piece of stiff wire hooked at one end to loop *g*, fastened to *b*, and at the other to a bend *d** in the indic The lower end of the indicator gives the reading on a paper millin scale *l*, gummed on to the mirror *p*. The mirror, of course, enable observer to avoid errors of parallax. The stand of the mirror is g to the lower collar. To appreciate the action of the levers, rega for the moment as fixed, then lowering *b* through a small distance *r* move *f* through a distance $s = \nu r$ at right angles to *mf*, where $\nu$ is ratio of the distance *mf* to the perpendicular let fall from *m* on line *gd* produced if necessary. If *md* be made perpendicular to when *f* is in the middle of the scale, the multiplier $\nu$ rem practically constant. This precaution was not always taken,

* This was a deeper bend than is shown in the figure.

FIG. 3.

wance is made for the resulting error. Two lever systems were
ıired, one for the outer ends, and the other for the inner ends of
needles passing through the ice. In Experiment 2 we used two
es and mirrors which enabled the readings to be taken with great
uracy. Afterwards we contented ourselves with one, which gave
te sufficient accuracy for any but homogeneous ice. In the first
experiments we used glass fibres, both for the indicator and
necting fibre, as we feared some slight motion of *f* might arise
n the "elastic recovery" of the wire. This was put to the test of
eriment. A long piece of the same kind of wire was bent sharply
an angle, and the two ends brought nearly into contact. It was
ıg over a nail, and the distance between the ends measured from
*e to time. The effect of* the gradual unbending of the angle would

in this case, owing to the greater length of the arms, be about twice as great as in the extension experiments, and yet it was found to be scarcely perceptible. For practical convenience in setting up the apparatus the wire was found immensely superior. The trouble of fixing in position a delicate arrangement of brittle glass fibres, in an awkward place like the back of the ice chamber behind the bar of ice, can hardly be realised by any one who has not tried it.

In the first few experiments the loops *m* and *g* were not used, and the indicator and connecting fibres were simply hooked over the needles *a* and *b*. And in Experiments 2, 3, 4, and 6, no efficient precautions were taken to prevent slipping along the needle. It is to be remarked, however, that any such slipping would produce an apparent contraction, and, owing to the sudden alteration of the rate of extension, any slipping of importance could hardly escape detection.* Such cases are either omitted or specially mentioned. The lever and counterpoise were found rather troublesome, and will probably be dispensed with next year, by putting the connecting fibre on the other side of the needle.

Our second apparatus, which we shall call the rough apparatus, was of much simpler construction. Instead of the collars we used two iron plates, each about 12 cm. square with a hole 2·5 cm. square in the centre. The bar to be tested was passed through the hole and frozen on to a block of ice on the other side of the plate. The upper plate was suspended by cords attached to holes at the corners, and from the lower plate was suspended by similar cords a bucket, in which various weights could be placed. In Experiments 3 and 4, the four cords were simply knotted together, and hung over an iron hook fastened to a single cord. But it was difficult in this way to ensure that the line of action of the tension should be the central line of the bar of ice, and we thought it likely that the bending in Experiment 4 was due to this cause, so we adopted the contrivance shown in fig. 4.

A is the upper iron plate, F the bar of ice attached to the block of ice E.† B is a wooden plate with holes at the corners and a hole at the centre, in exactly the same relative positions as the holes in the corners and the centre of the square hole in A. CCCC are four cords of equal length, and D the main cord by which the whole is upheld. When the arrangement is in equilibrium, the cords C will be vertical as well as the cord D, so the line of action of the tension, which is the central line of the cord D, will pass through the centre of the square hole in A, even though the two plates be not quite horizontal. The same remarks apply to a similar arrangement for the lower iron plate. If

* In almost every experiment far more readings were taken than are recorded below.

† This block was thicker than in the figure.

Fig. 4.

the bar be not attached accurately at right angles to the plates, it will take up a vertical position and the plates will be tilted. This contrivance was successful, for the icicle, which owing to its symmetrical formation would probably under uniform tension stretch equally on both sides, showed but small signs of bending. So we think it fair to conclude that in the later specimens the bending was due to their unsymmetrical structure.

In the later experiments (6, 8, 9 and 10) the apparatus was enclosed in a single box of wood about 3 cm. thick. The box was jacketed on the outside with a layer of hay about 5 cm. thick, covered with paper or felt. The cords, leading to the support and the weight, passed through holes in the top and bottom well plugged with cotton-wool. In all cases, except when the contrary is expressly mentioned, the bar of ice was wrapped in gutta-percha tissue to check the evaporation.

The polariscope was of the simplest possible form. The light transmitted by a sheet of thin paper was reflected at the polarising angle by a pile of three glass plates towards a Nicol prism supported in the same framework. With its aid it was easy to see the boundaries of the various crystals in a plate or bar of glacier ice, though not a trace of division could be detected with the naked eye, and with some difficulty the direction of the optic axes of a few of the larger crystals could be made out. In the bath ice the homogeneousness of the crystal could be readily tested, by watching the unchanged position of the rings and cross while the bar was moved across the field. In lake ice a half-inch plate, cut at right angles to the columns and viewed in the polariscope, showed a series of irregular polygons black, white, or grey, when the empty field was black. The *almost invariable absence* of colour proved that few or none of the

optic axes were nearly parallel to the length of the columns. That the axes, however, were not accurately perpendicular to the length of the columns, *i.e.*, horizontal in the original position on the lake, was shown by examining separate columns. After allowing the ice to thaw slightly, or better after leaving it in the rays of the sun for twenty minutes, the columns could be easily separated.

### *Detailed Account of the Experiments.*

It will be more convenient to describe all the experiments made with Main's apparatus first, than to keep to the chronological order.

*Experiment No.* 1. *Main's Apparatus.*—Measurements were taken with the cathetometer. The specimen was a square bar of ice, taken from the surface of a bath of water about a foot deep, and cut into shape with a knife. It was perfectly clear and free from bubbles. It was wrapped in gutta-percha tissue, which was not removed till the end of the next experiment. The "needles" were pieces of steel knitting needle. The area of the section was 8·1 sq. cm., and the tension 3·7 kilos. per sq. cm.

| Date. | Distance between needles. | Extension. | Temperature. | Difference between the two sides. | |
|---|---|---|---|---|---|
| | | | | Upper. | Lower. |
| | mm. | mm. | | mm. | mm. |
| Jan. 14, 11 h. .... | 163·93 | 0·0 | −3·0° | 4·1 | 4·6 |
| „ 16, 9 h. .... | 164·06 | +0·13 | −8·0 | 4·5 | 4·3 |
| „ 17, „ .... | 163·91 | −0·02 | −7·0 | 4·3 | 4·2 |
| „ 18, „ .... | 164·04 | +0·11 | −6·2 | 4·3 | 4·2 |
| „ 19, „ .... | 163·98 | +0·05 | −5·5 | 4·3 | 4·2 |
| „ 20, „ .... | 164·08 | +0·15 | −5·0 | .. | .. |
| „ „ „ .... | 164·01 | +0·08 | „ | .. | .. |
| „ 21, „ .... | 164·13 | +0·20 | −4·0 | 4·4 | 4·1 |

The hours in the first column are reckoned from midnight. The third column gives the extension observed, measured from the length at the first reading. The fourth column gives the temperature just before each reading. The maximum temperature during the whole period was −3·0° and the minimum −8·5°. The fifth column gives roughly the difference between the heights of the marks on the right and left ends of the upper needle, and the sixth column the same thing for the lower needle. These are added to show that a slight bending took place chiefly between the 14th and 16th. On removing the gutta-percha, at the end of the next experiment, a surface crack was found which may have occurred at the same time. Each reading

ı the micrometer scale of the cathetometer was taken twice, the lescope having been turned about the vertical axis in the interim. ıe two generally agreed. If not the mean was taken. On the 20th, wever, a second set of readings was taken, the telescope having en slid down the rod in the interim. Both determinations are ren.

The errors of a cathetometer reading have been already discussed. we allow 0·11 mm. as a possible error in each determination of length, the observations are consistent with no real extension. t taking the last two columns into consideration it seems probable t there was an extension of 0·1 mm. between the 14th aud 16th l none later. Even if the total extension had been 0·2 mm., this uld have corresponded to a mean extension per hour per length of cm. of only 0·0007 mm.

*Experiment No.* 2.—The same piece of ice was fitted up with glass icators and glass connecting fibres, the needles being the same as ore. Each indicator was provided with a mirror and scale set se up to it, so readings on the scale could be taken to 0·2 mm. t on the other hand there was a possibility of the indicators pping on the needles and thus occasioning a slight apparent conction. The multiplication on the outer side was 34, on the er 26. Thus an extension of 0·007 mm. could be detected. The ond, third, and fourth columns of the following table give the ensions, measured from the length at the time of the third ervation (for a reason mentioned below), and reduced to the protionate amount for a length of 10 cm. They are probably correct to )4 mm.

periment No. 2.—Main's Apparatus. Bath Ice. Length between Needles 16 cm. Tension 4·9 kilos. per square centimetre.

| Date. | Extension per 10 cm. | | | Temperature at the time. |
|---|---|---|---|---|
| | Outer. | Inner. | Mean. | |
| | mm. | mm. | mm. | |
| ı. 30, 10 h. 30 m. ....... | 0·016 | 0·000 | 0·008 | − 5·0° |
| „ 16 h. 30 m. ....... | 0·016 | 0·055 | 0·035 | .. |
| 31, 9 h. 15 m. ....... | 0·000 | 0·000 | 0·000 | −15·0 |
| „ 16 h. 30 m. ....... | −0·002 | 0·000 | −0·001 | −12·5 |
| b. 1, 9 h. 30 m. ....... | 0·000 | 0·019 | 0·009 | − 8·5 |
| 2, „ „ ....... | −0·002 | 0·002 | 0·000 | − 8·8 |
| „ 16 h. ............ | 0·007 | 0·022 | 0·014 | − 6·4 |
| 3, 9 h. 45 m. ....... | 0·007 | 0·022 | 0·014 | − 6·5 |
| 4, 12 h. ............ | 0·009 | 0·045 | 0·027 | − 8·7 |
| „ 18 h. 10 m. ....... | 0·007 | 0·050 | 0·028 | − 1·6 |
| 5, 9 h. 30 m. ....... | 0·007 | 0·048 | 0·027 | − 1·5 |
| „ 16 h. ............ | 0·007 | 0·048 | 0·027 | − 1·0 |

2 D 2

The temperature on the afternoon of the 30th was not taken, but the notebook contains a statement that it was colder than the morning.

Since the box was left open all night, the temperature given by the thermometer on the morning of the 31st may well have been rather lower than that of the ice. Between the 1st and 2nd, an apparent contraction of 0·017 mm. on one side took place without change of temperature. This looks as if the indicator had slipped. Making allowance for these, the mean extension from the 31st, 9 h. 15 m., to the 5th, 16 h., follows the temperature very fairly, considering the uncertainty of the latter. We have arranged the table to show this. But during the first six hours there was an expansion on one side of 0·088 mm. in actual magnitude, which we attribute to a slight yielding at the crack. Counting the contraction as a slip, and making no allowance for temperature, the mean rate during the whole 150 hours was 0·00019 mm. per hour per length of 10 cm.

If we suppose that the extension during the last five days was entirely due to temperature, and that the coefficient of expansion of the glass of the connecting fibre was 0·000009, we have between $-12·5°$ and $-8·5°$ a coefficient of linear expansion of ice of 0·000034, between $-8·5°$ and $-3·7°$ of 0·000060, and between $-3·7°$ and $-1·0°$ of 0·000009.

Into the complicated question of the expansion of ice with temperature we do not care to enter fully. We will merely cite two investigations. The best observations on the cubical coefficient seem to be those of Pettersson ("On the Properties of Water and Ice," '*Vega* Expedition,' vol. 2, Stockholm, 1883). We deduce from his figures the corresponding linear coefficients, supposing ice to be isotropic in this matter. With ice from ordinary distilled water he obtained 0·000053 between $-12°$ and $-2°$. This ice began to contract at some point between $-0·35°$ and $-0·25°$. With ice from the purest water he could obtain, the coefficient rose from 0·000055 between $-17°$ and $-10°$ to 0·000057 between $-4°$ and $-3°$, and then decreased, till it changed sign at some point between $-0·15°$ and $-0·03°$. Ice containing 0·014 per cent. of chlorine, in the shape of salts, began to contract at $-2·5°$. In these experiments the water was frozen in the dilatometer, so there was no chance of the impurities being expelled by the process of solidification as in the case of ice formed slowly on the surface of some depth of water. His purest water, however, was so good as to be seriously affected by boiling for a short time in a clean glass vessel.

The coefficient of linear expansion has been determined directly by Andrews ('Roy. Soc. Proc.,' June, 1886). He found 0·0000505 between $-18°$ and $-9°$, and 0·0000735 between $-9°$ and $-0°$. It is possible that the difference between the determinations of these two

experimentalists is owing to an unequal expansion of ice in different directions. At any rate, taken together, they are sufficient to explain our rough results, on the supposition that the extension of the last five days was entirely due to the rise of temperature.

The experiment was brought to a close by the bar breaking at a point above the upper needle, where it was not protected by gutta-percha tissue, and had become very thin through evaporation. The thickness had been reduced by this cause in three weeks from 2·85 cm. to 2·2 cm. The temperature, at which the fracture occurred, was between −0·5° and −1·0°, certainly not above the former. The breaking tension was 8·35 kilos. per sq. cm. There was a groove running right round the bar near the middle of its length, but no sign of a crack could be seen in the interior of the ice. This groove may have been caused by the outer layer cooling more rapidly than the interior. Under the polariscope no break in the continuity of the crystalline structure could be detected. The rings and cross were seen very plainly, and the direction of the optic axis appeared to be the same on both sides of the crack. It was perpendicular to the length of the bar and also to the needles. By a rough measure of the rings we found the difference between the two indices of refraction to be 0·0018. In quartz it is 0·0094; in Iceland spar 0·172.

*Experiment No. 5. Main's Apparatus.*—The specimen was a piece of glacier ice (B). The measurements were taken with the cathetometer. We had already found, in the other apparatus, that glacier ice would stretch, but we thought it desirable to confirm the fact with a different mode of measurement. So in this one case we used the cathetometer again, in spite of its disadvantages for this kind of work. The length between the needles was about 20 cm., the area of section 7·3 sq. cm., and the tension 2·7 kilos. per sq. cm. The second

Glacier Ice B. Length between Needles 20 cm. Tension 2·7 kilos. per sq. cm.

| Date. | Temperature at the time. | Interval. | Extension. | Rate per hour per 10 cm. | Temperature. Max. | Temperature. Mean. |
|---|---|---|---|---|---|---|
| | | hours. | mm. | mm. | | |
| Feb. 9, 9 h...... | −2·5° | 24 | 0·78 | 0·0160 | −2·5° | −3·5° |
| „ 10, „ ..... | −2·5 | | | | | |
| „ „ 16 h. 30 m. | .. | 16·25 | 0·44 | 0·0135 | −2·5 | −4·5 |
| „ 11, 8 h. 45 m. | −3 | | | | | |
| „ 12, „ | −4 | 32 | 0·53 | 0·0083 | −0·5 | −3·0 |
| „ 13, 16 h. 45 m. | −0·5 | | | | | |
| *Total*......... | .. | 72·25 | 1·75 | 0·0116 | .. | .. |

column gives the temperature just before each observation, the fourth the actual extension during the interval in millimetres, the error probably not exceeding 0·1 mm., the fifth the rate per hour per length of 10 cm., and the two last the maximum and mean temperatures during the interval.

On the 10th and 11th the ice broke at the collar, and had to be frozen together again. It will be noted that the rate of extension decreases with the time, more than can be explained by errors of observation, though the tendency of the temperature is to rise.

*Experiment No.* 7. *Main's Apparatus.*—The same piece of ice was used, cut a little shorter (glacier ice C), and fitted with wire indicators. Only one scale was used for the two indicators, so the readings cannot be trusted beyond 0·5 mm. on the scale. As the multiplication was generally about 16, this gives an error in the actual extensions, when small, not greater than 0·03 mm. When the extensions are large the error is greater, owing to an uncertainty of perhaps 10 per cent. in the multiplication. The "needles" were glass tubes. The length between the needles was 18 cm., and the area of section 7·3 sq. cm. The first column gives the time of each reading, the second the temperature at that time, the third the interval between two readings, the fourth and fifth the extension shown by the outer and inner indicators, the sixth the mean rate of extension per hour per length of 10 cm., the seventh the tension, the eighth, ninth, and tenth the maximum, minimum, and mean temperatures, during that interval.

On the 17th February the tension was increased by one-half, and the ice in consequence broke at the collar. It was frozen in again, and the tension reduced to the original value. On the 8th March an hour was occupied in readjusting the wire indicators. The sixth column shows a rapid decrease of speed for the first five days, followed by fluctuations due apparently mainly to the temperature, the rate at $-4°$ being about double that at $-9°$. An addition of one-half to the tension increased the rate 500 per cent. for the first two days of the change. This increased rate in its turn showed a tendency to sink, more or less counterbalanced by the rising temperature. The fourth and fifth columns show the curious way in which the more rapid extension alternates from one side to the other.

This piece of ice, taking the two experiments together, was under tension for twenty-five days, and extended altogether about 6 mm., *i.e.*, about 3 per cent. of its length. At the close of the experiment the divisions between two or three of the crystals at one point of the bar almost amounted to cracks, and at that point there was a decided twist in the bar, estimated at 10°. There were a great many bubbles in the ice, and the crystalline structure was very complex. There was no particularly large crystal.

Glacier Ice C. Length between Needles 18 cm.

| Date. | Temperature at the time. | Interval. | Extension. Outer. | Extension. Inner. | Rate per hour per 10 cm. | Tension kilos. per sq. cm. | Temperature. Max. | Temperature. Min. | Temperature. Mean. |
|---|---|---|---|---|---|---|---|---|---|
| | | hours. | mm. | mm. | mm. | | | | |
| Feb. 16, 10 h. | −5·0° | | | | | | | | |
| „ 17, 9 h. | −7·3 | 23 | 0·25 | 0·31 | 0·0068 | 2·55 | −2·5° | − 7·3° | −4·5° |
| „ „ 15 h. 15 m. | −5·8 | | | | | | | | |
| „ 18, 9 h. 30 m. | −8·5 | 18·25 | 0·19 | 0·09 | 0·0042 | „ | −5·5 | − 8·5 | −7·0 |
| „ 21, 8 h. 45 m. | −6·0 | 71·75 | 0·09 | 0·25 | 0·0013 | „ | −6·0 | −12·0 | −9·0 |
| „ 23, „ | −8·5 | 48 | 0·14 | 0·12 | 0·0015 | „ | −4·8 | − 8·5 | −7·0 |
| „ 25, 9 h. | −7·2 | 48·25 | 0·07 | 0·19 | 0·0015 | „ | −7·2 | − 8·8 | −7·8 |
| „ 27, 9 h. 15 m. | −4·5 | 48·25 | 0·15 | 0·22 | 0·0021 | „ | −4·0 | − 7·2 | −5·0 |
| „ 29, 9 h. | −4·5 | 47·75 | 0·32 | 0·19 | 0·0029 | „ | −2·8 | − 4·5 | −3·8 |
| Mar. 2, 8 h. 45 m. | −6·5 | 47·75 | 0·21 | 0·12 | 0·0018 | „ | −4·5 | − 6·5 | −5·6 |
| „ 4, 9 h. | −5·2 | 48·25 | 0·97 | 0·94 | 0·0110 | 3·85 | −3·5 | − 6·5 | −5·2 |
| „ 6, „ | −7·0 | 48 | 0·55 | 0·62 | 0·0068 | „ | −5·2 | − 7·5 | −6·5 |
| „ 8, „ | −5·7 | 48 | 0·56 | 0·78 | 0·0078 | „ | −5·0 | − 7·0 | −6·0 |
| „ „ 10 h. 15 m. | | | | | | | | | |
| „ 9, 16 h. 30 m. | −2·4 | 30·25 | 0·27 | 0·72 | 0·0090 | „ | −2·4 | − 5·7 | −3·8 |
| Total | .. | 527 | 3·8 | 4·55 | 0·0044 | .. | .. | .. | .. |

We now come to the experiments made with the rough apparatus. At first it fully deserved the name, but later on, viz., in Experiments 6, 8, 9, and 10, the results were quite as trustworthy as in the more elaborate arrangement.

*Experiment No. 3. Rough Apparatus.*—The specimen was a circular cylinder frozen in Main's mould, about 20 cm. between the needles.

The area of section was 6 sq. cm., and the tension 4 kilos. per sq. cm. The measurement was taken with glass indicators. A long straight glass fibre was used as indicator, bent at one end to hook under the lower needle, and supported in a nearly horizontal position by a glass connecting fibre hooked over the upper needle. The vertical scale was attached to an arm projecting from the upper iron plate.

During the first 22·5 hours the ice extended 3·7 mm. on the outer side, and contracted 0·75 mm. on the inner side. During a subsequent six hours it extended 1·7 mm. on the outer side and 0·6 mm. on the inner. The mean rate per hour per length of 10 cm. was therefore 0·046 mm. The temperature is not known with any certainty. This ice was never examined under the polariscope, but owing to the mode of formation described fully at the end of the paper, we may be certain the structure was in the highest degree irregular. It was probably, however, tolerably symmetrical about the axis, so the bending may be attributed to the eccentric application of the pull.

*Experiment No. 4. Rough Apparatus.*—The specimen was a piece of glacier ice (A), composed of perhaps a dozen "grains" very irregularly arranged, the axes of some being at right angles, of others parallel, to the length. Distance between needles about 22 cm. The area of the section is a little uncertain, as it was not measured *in situ*, and the ice was not protected from evaporation. It may be taken as 6·5 sq. cm., and the tension as 1·66 kilo. per sq. cm. The ice was subjected to tension for about eighty-five hours altogether, but we only give the results for the last twenty-seven, as at first the indicators appear to have slipped, and, after precautions had been taken to prevent slipping the two indicators happened to come in contact. The indicators were arranged as in the last experiment, but the readings were improved by attaching a mirror to the scale. The multiplication was about 30, and the extensions may be trusted to 0·03 mm. The first column in the annexed table gives the time of each reading, the second the temperature at that time, the third the interval between two readings, the fourth and fifth the actual extensions measured by the outer and inner indicators in that interval, and the sixth the rate per hour per length of 10 cm.

The temperatures are somewhat uncertain, as the ice was not enclosed in a box, and the temperature of the room was very far from being uniform. The last four temperatures were taken by a thermometer hung close by the ice and on the same level. The minimum of the night by this thermometer was −3·3°. The high temperature at 21 h. 15 m. was due to the window of the room having been nearly closed. It was then thrown wide open, so the temperature must have soon fallen again. So the interval before this reading, 0·0°, would probably be much warmer on the average than the subsequent

Glacier Ice A. Length between Needles, 22 cm. Tension 1·66 kilos. per sq. cm.

| Date. | Temperature. | Interval. | Extension. | | Rate per hour per 10 cm. |
|---|---|---|---|---|---|
| | | | Outer. | Inner. | |
| | | hours. | mm. | mm. | mm. |
| Feb. 3, 9 h. ............ | −2·5° | | | | |
| | | 3·75 | 0·23 | 0·02 | 0·015 |
| „ „ 12 h. 45 m. ...... | −1·0 | | | | |
| | | 3·75 | 0·19 | 0·01 | 0·013 |
| „ „ 16 h. 30 m. ...... | −4·0 | | | | |
| | | 4·75 | 0·35 | 0·10 | 0·022 |
| „ „ 21 h. 15 m. ...... | 0·0 | | | | |
| | | 11·25 | 0·53 | 0·16 | 0·014 |
| „ 4, 8 h. 30 m. ...... | −3·0 | | | | |
| | | 3·50 | 0·19 | 0·08 | 0·018 |
| „ „ 12 h. ............ | −1·0 | | | | |
| Total ........... | .. | 27·0 | 1·49 | 0·37 | 0·0156 |

interval. Thus the sixth column shows that the ice became more plastic as it neared the thawing point. The unequal extensions in the fourth and fifth columns may well have been due to eccentric application of the tension.

*Experiment No.* 6. *Rough Apparatus.*—The specimen was an icicle trimmed with a knife to an uniform circular section. The apparatus was greatly improved. The new mode of suspension was adopted, specially arranged, as described above, to ensure the tension acting along the central line of the bar. The indicators were hooked over the top needle and bent at right angles so as to point downwards, as in Main's apparatus. They were of glass, and no thoroughly efficient means was taken to prevent slipping along the needle, but we do not think any slipping can have taken place during the observations quoted below. The whole apparatus was enclosed in a jacketed box—which was, however, generally left open at night—and a centigrade thermometer, graduated to tenths, was hung in the box on a level with the middle of the ice.

In the table the fourth and fifth columns give the actual extensions during each interval, which may be trusted to 0·015 mm., and the sixth column the mean rate of extension per hour per length of 10 cm. The second column gives the reading of the thermometer at the time of the observation, and the last two columns the maximum and mean temperatures of the ice during each interval. These are tolerably accurate, as many observations were taken besides those here quoted. The ice was not protected from evaporation, so the

Icicle. Length between Needles, 19 cm.

| Date. | Temperature at the time. | Interval. | Extension. | | Rate per hour per 10 cm. | Tension, kilos. per square centimetre. | Temperature. | |
|---|---|---|---|---|---|---|---|---|
| | | | Outer. | Inner. | | | Maximum. | Mean. |
| | | hours. | mm. | mm. | | | | |
| Feb. 11, 22 h. ......... | −2·0° | 10·5 | 0·035 | 0·070 | 0·0026 | 2·0 | −2·0° | −2·3° |
| " 12, 8 h. 30 m. ..... | −2·7 | 7·75 | 0·0 | 0·045 | 0·0015 | 2·1 | −0·7 | −1·3 |
| " " 16 h. 15 m. ..... | −0·7 | 16·5 | 0·070 | 0·105 | 0·0037 | 2·1 | 0·0 | −0·2 |
| " 13, 8 h. 45 m. ..... | +0·5 | 11·75 | .. | .. | .. | 0 | 0·0 | 0·0 |
| " " 20 h. 30 m. ..... | +0·5 | 13·0 | 0·100 | 0·0 | 0·0024 | 2·3 | 0·0 | −1·5 |
| " 14, 9 h. 30 m. ..... | −3·0 | 7·0 | 0·075 | 0·020 | 0·0035 | 2·4 | −0·7 | −2·0 |
| " " 16 h. 30 m. ..... | −0·7 | 16·5 | 0·165 | 0·070 | 0·0032 | 2·5 | 0·0 | −0·8 |
| " 15, 9 h. ......... | +0·6 | 4·75 | 0·035 | 0·040 | 0·0041 | 2·6 | 0·0 | 0·0 |
| " " 13 h. 45 m. ..... | +0·5 | | | | | | | |
| Total ............ | .. | 78 | 0·48 | 0·36 | 0·0028 | .. | .. | .. |

section gradually diminished, and the tension consequently increased, as given in the seventh column. The mean section was about 4·1 sq. cm. The ice was under tension for twenty-four hours previous to the observations given below, but during this time the indicators seem to have slipped.

The weight was removed for twelve hours on the 14th owing to the thaw. It is curious to notice how irregularly the extension is divided between the two sides; the ice bends first one way then the other. The fluctuations in the mean rate of extension seem mainly due to the temperature. During thirteen hours at a temperature between $-1·5°$ and $-3·0°$ the rate was 0·0028, while during thirty-eight hours at a temperature above $-0·7°$ the rate was 0·0034. The ice was full of minute bubbles, though not in sufficient quantity to make it quite opaque. The component crystals were very small, less than a millimetre in diameter, and with optic axes arranged quite irregularly.

*Experiment No.* 8. *Rough Apparatus.*—The specimen was a piece of glacier ice (D). The wire indicators and connecting fibres were hooked through wire loops firmly fastened to the glass needles embedded in the ice, so there was no possibility of slipping. The multiplication was about 22, so the small extensions are accurate to 0·02 mm. The area of section was 6·3 sq. cm. The table is arranged as in the last experiment (6).

Thus the whole extension in three and a half days was more than 4 per cent. of the length. At 20 h. 15 m. the inner indicator had moved off the scale against a stop, so the extension was probably rather greater, certainly not less than that given. The extension at a particularly low temperature, mentioned in the general summary, was between February 18th, 21 h., when the temperature was $-9·0°$, and February 19th, 9 h. 15 m. There was a contraction on the outer side during this interval of 0·01 mm., and an extension on the inner side of 0·23 mm., so the mean rate per hour per 10 cm. was 0·0065 mm.

It should be mentioned that the points on the glass needles, where the indicators were attached, were not quite close to the ice, but at the distance of a centimetre perhaps. Hence, while the mean rate is correctly given, the extension on the inner side of the bar is exaggerated, and that on the outer side made too small. Taking the ice as 2·5 cm. thick, this consideration leads to the result that the total extension of the outer face of the bar was 2·9 mm., of the inner face 9·7 mm.

This experiment shows how completely the plasticity depends on changes in the internal structure of the ice. Thus, for the first two days we find, under a slight stress, a moderate rate showing some *tendency to decrease* more rapidly than can be easily attributed to *the fall of temperature.* An increased tension produces as usual a

[redacted]



Glacier Ice D. Length between Needles, 14 cm.

| Date. | Temperature at the time. | Interval. | Extension. Outer. | Extension. Inner. | Rate per hour per 10 cm. | Tension, kilos. per sq. cm. | Temperature. Max. | Temperature. Mean. |
|---|---|---|---|---|---|---|---|---|
| | | hours. | mm. | mm. | | | | |
| Feb. 18, 10 h.......... | − 6·7° | | | | | | | |
| " " 17 h.......... | − 6·0 | 7·0 | −0·03 | 0·27 | 0·0122 | 1·45 | −5·0° | − 6·0° |
| " 19, 9 h. 15 m. .... | −11·0 | 16·25 | −0·04 | 0·28 | 0·0054 | " | −6·0 | −10·0 |
| " " 19 h. 15 m. .... | − 7·7 | 10·0 | +0·01 | 0·19 | 0·0071 | " | −7·0 | − 8·0 |
| " 20, 9 h.......... | − 6·5 | 13·75 | −0·03 | 0·32 | 0·0075 | " | −6·0 | − 7·0 |
| " " 12 h. 45 m..... | − 5·4 | 3·75 | −0·06 | 0·33 | 0·026 | 2·55 | −5·4 | − 6·0 |
| " " 14 h.......... | − 4·7 | 1·25 | −0·01 | 0·37 | 0·103 | " | −4·7 | − 5·0 |
| " " 16 h. 30 h. .... | − 4·2 | 2·50 | −0·08 | 0·99 | 0·128 | " | −4·2 | − 4·5 |
| " " 20 h. 15 m..... | − 4·6 | 3·75 | −0·09 | 1·20? | 0·105? | " | −4·2 | − 4·4 |
| " 21, 8 h. 45 m. .... | − 3·7 | 12·5 | +0·06 | 0·29 | 0·010 | 1·03 | −3·7 | − 4·2 |
| " " 9 h. 15 m..... | − 3·7 | 0·5 | −0·01 | 0·33 | 0·228 | 2·50 | −3·7 | − 3·7 |
| " " 10 h. ........ | − 3·0 | 0·75 | +0·08 | 0·95 | 0·485 | " | −3·0 | − 3·3 |
| " " 11 h. ........ | − 2·9 | 1·0 | +0·15 | 1·68 | 0·65 | " | −2·9 | − 3·0 |
| " " 11 h. 20 m..... | − 2·2 | 0·33 | +0·39 | 1·10 | 1·58 | " | −2·2 | − 2·5 |
| " " 11 h. 30 m. .... | − 2·0 | 0·16 | +0·12 | 0·77 | 1·88 | " | −2·0 | − 2·1 |
| " " 12 h. 15 m..... | − 2·0 | 0·75 | +0·05 | 0·70 | 0·35 | 1·80 | −2·0 | − 2·0 |
| " " 13 h. 45 m..... | − 1·7 | 1·5 | 0·0 | 1·10 | 0·265 | " | −1·7 | − 1·7 |
| " " 16 h. ........ | − 2·0 | 2·25 | −0·06 | 0·77 | 0·110 | " | −1·7 | − 1·8 |
| " " 21 h. 30 m..... | − 3·7 | 5·5 | −0·16 | 0·83 | 0·043 | " | −2·0 | − 2·8 |
| Total............ | .. | 83·5 | +0·21 | 12·47 | .. | .. | .. | .. |

large increase in the velocity. But it has further the remarkable effect of transforming a slow retardation into a rapid acceleration. A light tension now reduces the velocity to nearly the old figure. But as soon as the former tension is restored, the acceleration continues till the velocity reaches nearly 2 mm. an hour. It is true that this acceleration was attended by a rising temperature, but it seems far too great to be attributed to that alone. We may fairly conclude *that the* process of extension itself has sometimes the effect of *increasing* the apparent plasticity. Reducing the tension by one-*third brought* down the velocity at once by four-fifths, and, strange

to say, impressed a gradual retardation in spite of a rising temperature. It would thus appear that in this case, while a rapid extension increased the plasticity, a gradual extension had the effect of diminishing it. This is an anomalous result, but it must be remembered that we are measuring the sum of a large number of independent actions. The behaviour of the whole is probably much more complicated than that of any one of the individuals.

Being curious to see the effect of great tension, we applied 4·2 kilos. per sq. cm. This brought the experiment to an end, for after half a minute the ice gave way. It was found broken both at the lower collar and at a point below the upper needle, where we had previously noticed a crack extending part of the way across the bar. At which point it broke first we cannot say. The bar was examined at the end of the experiment. It was nearly straight in spite of one side having extended so much more than the other. It contained several large bubbles, one perhaps 2 cm. long, drawn out into very irregular shapes, which seemed to show this piece had suffered great distortion while it still formed part of the glacier. It contained part of a very large crystal which composed, perhaps, one third of the whole bar, and ran three quarters of the length between the needles. This crystal occupied one of the angles adjacent to the inner face, which extended so much. Its optic axis was inclined at perhaps 70° to the length of the bar.

*Experiment No. 9. Rough Apparatus.*—The specimen was a bar of lake ice, with the crystalline columns parallel to the length of the bar. The section was 8 sq. cm. in area. The arrangements were the same as in the last experiment (8). The extensions are so small that the deduced rate during each interval would be very inaccurate. We have therefore given, in the second, third, and fourth columns of the table, the extensions measured from the length at the time of the first reading and reduced to the proportionate value for a bar 10 cm. long. They are probably correct to 0·01 mm. The fifth column gives the temperature shown by the thermometer at each reading; and the next three the maximum, minimum, and mean temperatures of the ice during each interval, estimated from a large number of observations not quoted.

Previously to 15 h., February 28th, the ice must have been thawing, probably for about an hour. The weight was removed for the next three hours. The total extension during 208 hours per length of 10 cm. was 0·145 mm. on the outer side, and 0·048 mm. on the inner, giving a mean rate per hour of 0·00046 mm. The mean rate during the first 168 hours was 0·00039 mm., and during the last 40 with the heavier weight 0·00076 mm., notwithstanding a slightly lower mean temperature. But these rates were so small as to be beyond our means of accurate measurement.

Lake Ice parallel to Columns. Length between Needles, 16 cm.

| Date. | Extension per 10 cm. | | | Temperature. | | | | Tension kilos. per sq. cm. |
|---|---|---|---|---|---|---|---|---|
| | Outer. | Inner. | Mean. | At the time. | Maximum. | Minimum. | Mean. | |
| | mm. | mm. | mm. | | | | | |
| Feb. 23, 14 h. | 0·0 | 0·0 | 0·0 | −5·0° | | | | |
| | | | | | −4·0° | −8·0° | −7·0° | 2·1 |
| „ 26, 9 h. | 0·036 | 0·017 | 0·026 | −4·0 | | | | |
| | | | | | −2·0 | −4·5 | −3·2 | 2·1 |
| „ 28, 8 h. | 0·072 | 0·036 | 0·054 | −4·1 | | | | |
| | | | | | 0·0 | −4·1 | −2·0 | 2·1 |
| „ „ 15 h. | 0·085 | 0·024 | 0·054 | +2·0 | | | | |
| | | | | | | | | 0·0 |
| „ „ 18 h. | 0·085 | 0·024 | 0·054 | −1·0 | | | | |
| | | | | | −1·0 | −9·0 | −5·0 | 2·1 |
| *Mar. 1, 17 h. | 0·108 | 0·024 | 0·066 | −5·0 | | | | |
| | | | | | −4·0 | −7·0 | −5·5 | 2·8 |
| „ 3, 9 h. | 0·145 | 0·048 | 0·096 | −6·0 | | | | |

* There were 29 days in February this year.

camining the bar at the end of the experiment, we counted about y columns in a section, most of which ran the full length of the The largest had a sectional area of about 35 sq. mm.

*xperiment No.* 10. *Rough Apparatus.*—The specimen was a bar of ice, with the crystalline columns running obliquely across at an e of 45° to the length of the bar. The area of section was sq. cm. The indicators, &c., were arranged as before. The tem-ture at the time of observation, and the minimum temperature e observed; the maximum and mean temperatures are estimated. fourth and fifth columns give the actual extension during each rval. They are probably correct to 0·02 mm., as the multiplica- was 35.

'he rate shows a decided tendency to decrease, only slightly cked by the rise of temperature. The glass needles were put at ht angles to the columns as well as to the length of the bar.

Lake Ice oblique to the Columns. Length between Needles, 11·5 cm. Tension, 2·75 kilos. per sq. cm.

| Date. | Temperature at the time. | Interval. | Extension. | | Rate per hour per 10 cm. | Temperature. | | |
|---|---|---|---|---|---|---|---|---|
| | | | Outer. | Inner. | | Maximum. | Minimum. | Mean. |
| | | hours. | mm. | mm. | | | | |
| Mar. 5, 9 h. 30 m.… | −6·0° | | | | | | | |
| | | 6·5 | 0·09 | 0·42 | 0·034 | −5·6° | −6·1° | −5·8° |
| „ „ 16 h.……… | −5·6 | | | | | | | |
| | | 17 | 0·12 | 0·42 | 0·014 | −5·6 | −7·8 | −6·7 |
| „ 6, 9 h.……… | −7·8 | | | | | | | |
| | | 8 | 0·10 | 0·16 | 0·014 | −5·6 | −7·8 | −6·7 |
| „ „ 17 h.……… | −5·6 | | | | | | | |
| | | 16 | 0·10 | 0·26 | 0·010 | −5·6 | −6·7 | −6·0 |
| „ 7, 9 h.……… | −6·7 | | | | | | | |
| | | 9 | 0·12 | 0·14 | 0·013 | −3·3 | −6·7 | −5·0 |
| „ „ 18 h.……… | −3·3 | | | | | | | |
| | | 15 | 0·17 | 0·21 | 0·011 | −3·3 | −5·8 | −4·6 |
| „ 8, 9 h.……… | −5·6 | | | | | | | |
| | | 8 | 0·14 | 0·11 | 0·014 | 0·0 | −5·6 | −2·8 |
| „ „ 17 h.……… | 0·0 | | | | | | | |
| Total……… | .. | 79·5 | 0·85 | 1·72 | 0·015 | .. | .. | .. |

We shall now describe the experiments on compression. An oblong ce of thick plate glass was laid on the table, and on it were placed ree square blocks of ice, at the angles of an equilateral triangle out 9 cm. in the side. On the ice was laid a second piece of plate ss similar to the first, and pressure applied by means of a lever at oint immediately over the centre of the triangle. Measurements re taken with callipers of the distance between the plates at three nts on the edge, such that each point lay on a line through the tre and one angular point of the triangle. By drawing a diagram scale, it was not difficult to deduce from these measurements the lding of each block of ice. To prevent slipping, we found it essary and sufficient to freeze a slip of paper on each end of a ck of ice. A maximum thermometer was placed on the table close the plates, and covered over with the same cloth, so that it bably gave the temperature of the ice within a degree. The rizontal section of each block was 7·5 sq. cm. in area. The fourth, h, and sixth columns give the actual contraction of the blocks ring each interval. They are correct probably within 0·02 mm. ch measurement with the callipers was repeated, and the two dings seldom differed more than 0·02 mm.

Pressure had been applied for one day previous to those here given, t owing to an accident, its magnitude was rather uncertain. The markable difference between the plasticity of three specimens glacier ice is well shown, though in this case all three pieces re from the same lump. After the experiment they were examined der the polariscope. All three were composed of smallish grains eraging perhaps 7 mm. in diameter. The increase of plasticity for ise in temperature from $-6°$ to $-3°$ is very striking in all three ces.

*Experiment No. 2 on Compression.*—In this three pieces of lake ice re arranged as in the last experiment. The crystalline columns re vertical, so that the pressure was applied in a direction parallel them. The horizontal section of each piece was 7 sq. cm. The rth, fifth, and sixth columns of the table give the contractions ring each interval, calculated from the readings actually taken, as plained in the description of the last experiment. They are proly accurate to 0·02 mm. It may be mentioned that the totals are lculated to an extra place of decimals, which explains the slight screpancy observable.

Compression Experiment No. 1. Three pieces of Glacier Ice. Initial Length, 2·9 cm. Pressure 3·2 kilos. per sq. cm.

| Date. | Temperature at the time. | Interval. | Contraction. E. | Contraction. F. | Contraction. G. | Temperature. Maximum. | Temperature. Mean. |
|---|---|---|---|---|---|---|---|
| | | hours. | mm. | mm. | mm. | | |
| Feb. 21, 9 h. .......... | − 4·3° | | | | | | |
| | | 24 | 0·37 | 0·30 | 0·06 | −2·8° | −6·4° |
| „ 22, „ .......... | −12·0 | | | | | | |
| | | 24 | 0·05 | 0·09 | 0·02 | −4·0 | −7·0 |
| „ 23, „ .......... | −11·4 | | | | | | |
| | | 24 | 0·09 | 0·17 | 0·00 | −4·4 | −7·5 |
| „ 24, „ .......... | − 9·4 | | | | | | |
| | | 24 | 0·06 | 0·10 | −0·02 | −4·7 | −6·4 |
| „ 25, „ .......... | − 6·4 | | | | | | |
| | | 24 | 0·61 | 1·24 | 0·18 | −2·8 | −4·0 |
| „ 26, „ .......... | − 2·8 | | | | | | |
| Total ......... | .. | 120 | 1·18 | 1·92 | 0·25 | .. | .. |
| Rate per hour per 10 cm. { 2nd, 3rd, and 4th days............. | | | 0·0096 | 0·0172 | 0·000 | .. | −7·0 |
| Rate per hour per 10 cm. { 5th day......................... | | | 0·088 | 0·178 | 0·026 | .. | −4·0 |

Compression Experiment No. 2. Lake Ice compressed along the Columns. Length, 3·4 cm. Pressure, 3·7 kilos. per sq. cm.

| Date. | Temperature at the time. | Interval. | Contraction. | | | Temperature. | |
|---|---|---|---|---|---|---|---|
| | | | A. | B. | C. | Maximum. | Mean. |
| | | hours. | mm. | mm. | mm. | | |
| Mar. 3, 22 h. ......... | −8·6° | | | | | | |
| ,, 4, 9 h. ......... | −8·3 | 11 | 0·00 | 0·02 | 0·02 | −3·9° | −6·0° |
| ,, 5, ,, ......... | −6·7 | 24 | 0·01 | 0·02 | 0·01 | −6·7 | −7·0 |
| ,, 6, ,, ......... | −7·8 | 24 | 0·00 | 0·00 | 0·01 | −4·7 | −6·0 |
| ,, 7, ,, ......... | −5·6 | 24 | 0·00 | 0·00 | 0·01 | −4·2 | −5·8 |
| Total ........... | .. | 83 | 0·005 | 0·035 | 0·050 | .. | .. |
| Rate per hour per 10 cm. ......... | .. | .. | 0·0002 | 0·0012 | 0·0018 | .. | .. |

Thus the yielding of one piece was well within the errors of observation, of the other two only just perceptible with the instrument employed, and this small yielding may well have taken place entirely in the thin layer of irregular ice with which the paper was attached.

In the early part of the winter we made, as already mentioned, a large number of experiments on obtaining ice in the mould* free from air bubbles. We were ultimately successful, and, though our experiments proved to be of little use for their immediate object, they are of some permanent interest as tests of various methods of obtaining air-free water, so we shall describe a few typical ones. Main, the previous winter, boiled the water and let it freeze, then melted it in the mould, boiled it, and let it freeze again. The result was clear ice, except for "a small core of minute bubbles up the axis of the cylinder." By Main's advice we procured an air-pump adapted to exhaust the air from the mould. Between the pump and the mould was a good stopcock, which would maintain the vacuum for several hours. When in good order the pump would boil water at 40° C., or below. We found that this degree of exhaustion was far from removing all the air, even when applied for five hours. Boiling for half an hour, cooling *sub vacuo*, and freezing at atmospheric pressure under oil was more successful, but not satisfactory. We froze the water at atmospheric pressure to make the bubbles small, having placed a layer of oil on the top to prevent air entering. The next method proved much more effectual. We kept water *sub vacuo* for twenty-four hours at about 70° C., and let it cool *sub vacuo*, only admitting air after the freezing had begun. There were a few exceedingly small bubbles visible at one end of the rod of ice. Thawing this *sub vacuo* and keeping it again for twenty-four hours *sub vacuo* at 70° C., we got rid of the last traces of air in the rod, though there were a few in the large cone of ice.

[We conclude that, to free water from air, it should first be boiled till most of the dissolved air has escaped, and then left for a considerable time without permitting any air to have access to its surface. Boiling should be repeated at intervals to remove the air, which gradually escapes from the water and mingles with the aqueous vapour in the space above. It is probable that a high temperature quickens the process.—July 6, 1888.]

The utter irregularity of the crystalline structure of the mould ice is an obvious consequence of the mode of formation. The first ice formed, no doubt, is a layer on the surface, but the centre of this is soon broken though by water forced up from below, owing to the expansion in freezing. So what we observed in its various stages was

* This was the iron mould used by Main to form a round column of ice 2·8 cm. in diameter and 24 cm. in length, with a conical expansion at the lower end of perhaps half the volume of the column.

a ring of ice formed at the surface, which gradually extended down the sides and towards the centre, till we had a long tube of ice thinning out towards the lower end joined on to a case of ice, lining the inside of the cone. The tube grew thicker and thicker, till it became a solid bar. When a piece of sheet india-rubber was laid on the surface (to prevent air entering), it was frozen firmly to the sides of the mould, while the centre was pushed upwards into the shape of a bee-hive, till at last it burst. It was curious to find the india-rubber with the middle part drawn out into a long tube with torn edges, firmly imbedded in the ice at some little distance from the end.

In conclusion, we wish to express our thanks to Dr. Main for the use of his special stretching machine, and of the various thermometers, callipers, and much other apparatus, which he has generously placed at our service.

In case any reader of this paper should be kind enough to offer us any useful suggestions, or on the other hand should desire further information on any point, we give here the permanent address of one of the authors, James C. McConnel, Brooklands, Prestwich, Manchester, England. We may add that copies of papers bearing on the subject would be particularly acceptable.

## V. "On the Organisation of the Fossil Plants of the Coal-measures. Part XV." By W. C. WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Received June 13, 1888.

(Abstract.)

The author describes and figures a series of specimens which throw new light upon Corda's two genera *Zygopteris* and *Anachoropteris*, as they are adopted by M. Renault, but which specimens show that both these genera can no longer be retained, even by those who approve of such multiplications of ill-defined genera. He proposes, therefore, the abandonment of *Anachoropteris* and the retention of *Zygopteris*, so that "*Zygopteroid*" may be employed as a descriptive adjective in connexion with some specially remarkable forms of petiolar vascular bundles. Under the name of *Rachiopteris hirsuta*, a new group of freely branching stems or rhizomes are figured and described, characterised by having the exterior of their bark abundantly clothed, especially in what appear to be the younger shoots, with remarkably large curved multicellular hairs, closely resembling those similarly located in the young shoots of the Marsileæ; numerous *cylindrical roots* radiate from these axial organs. Under *the provisional name of Rachiopteris verticillata* attention is also

called to some curious roots, the secondary branches of which are given off in regular verticils; besides these plants two other distinct kinds of roots are described, in each of which the cortical parenchyma is characterised by containing numerous lacunæ of the type so common amongst aquatic and semi-aquatic forms of vegetation—*e.g.*, *Nymphœa.* All the above objects are from the Lower Carboniferous beds at Halifax.

## VI. "Effects of Different Positive Metals, &c., upon the Changes of Potential of Voltaic Couples." By G. Gore, F.R.S.

Received June 13, 1888.

The following effects upon the minimum change of potential of a voltaic couple in water ('Roy. Soc. Proc.,' May 26, 1888), and upon the change of potential attending variation of strength of its exciting liquid (*ibid.*, May 31, 1888), were obtained by varying the kind of positive (and of negative) metal of the couple, and by employing different galvanometers. The measurements were made by the method of balance, with the aid of a thermo-electric pile* ('Birmingham, Phil. Soc. Proc.,' vol. 4, p. 130), and the numbers have been corrected for errors caused by absorption of hydrogen by the platinum. The water employed was ordinary distilled water, redistilled after addition of a minute amount of sulphuric acid, and was quite free from ammonia.

Table I.—Mg + Pt + HCl in 465 grains of Water at 17° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·15 | 1·7119 | 0·0458 | 1·6861 |
| 0·13563 | 1·6946 | 0·0308 | 1·6804 |
| 0·12066 | 1·6804 | 0·0158 | 1·6746 |
| 0·10569 | " | 0·0009 | 1·5946 |
| 0·09072 | " | 0·0008 | 1·566 |
| 0·07575 | 1·6861 | water | " |
| 0·06078 | " | .. | .. |

With an ordinary astatic galvanometer of 100 ohms resistance, the smallest proportion of the anhydrous acid required to change the potential, lay between 1 part in 516,666 and 570,000 parts of water;

* *This instrument is manufactured by Messrs. Nalder, Brothers, Horseferry Road, Westminster.*

but with a Thomson's reflecting one of 3040 ohms resistance, it was between 775,000 and 930,000.

The effects obtained with zinc as a positive metal have already been given ('Roy. Soc. Proc.,' May 31, 1888). With that metal and the astatic galvanometer the minimum proportion of acid required to change the potential lay between 1 part in 9,300,000 and 9,388,185 parts of water; but with the reflecting one it lay between 1 in 15,500,000 and 23,250,000.

Notwithstanding the electromotive force of magnesium is so much larger than that of zinc in the very dilute acid, the minimum proportion of the acid required to destroy the balance was very much smaller with zinc than with magnesium, and the increase of electromotive force was more rapid with zinc than with magnesium. The minimum proportion of acid required to change the potential with magnesium ('Roy. Soc. Proc.,' May 26, 1888), or with zinc, was nearly the same, whether the couple was balanced by a precisely similar one or by the thermo-electric pile. The order of variation of electromotive force by change of strength of the liquid was very similar with zinc to what it was with magnesium, and the curves generated by plotting the results were much alike.

Table II.—Cd + Pt + HCl in 465 grains of Water at 17·5° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 0·15 | 0·9494 | 0·07575 | 0·9108 | 0·0009 | 0·7678 |
| 0·13563 | 0·9108 | 0·06078 | 0·9251 | 0·00081 | ,, |
| 0·12066 | ,, | 0·04581 | 0·9427 | 0·00073 | 0·7478 |
| 0·10569 | ,, | 0·03084 | ,, | water | ,, |
| 0·09072 | ,, | 0·01584 | 0·9451 | .. | .. |

With the astatic galvanometer, the smallest proportion of acid required to alter the balance was between 1 in 574,000 and 637,000; but with the reflecting galvanometer it was between 1 in 1,162,500 and 1,550,000. The order of change, or curve of electromotive force by variation of strength of liquid, was somewhat similar with cadmium to what it was with zinc and magnesium.

Table III.—Al + Pt + HCl in 465 grains of Water at 16·5° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·15 | 0·9003 | 0·06078 | 0·8431 |
| 0·13563 | 0·866 | 0·04581 | 0·8288 |
| 0·12066 | 0·8517 | 0·0384 | 0·823 |
| 0·10569 | 0·8345 | 0·03084 | 0·8145 |
| 0·09072 | 0·8431 | water | ,, |
| 0·07575 | 0·8517 | .. | .. |

With the astatic galvanometer, the minimum proportion of acid required to change the potential lay between 1 part in 12,109 and 15,000 parts of water; but with the reflecting one it was between 1 in 42,568 and 46,500. The curve of variation of electromotive force, by uniform change of strength of liquid, was less regular than with either zinc or magnesium, but presented certain points of similarity with the curves of zinc, magnesium, and cadmium.

The following table shows the proportions of the acid required to upset the balance of each of the preceding couples in water:—

Table IV.

With the Astatic Galvanometer.

| | | | | | | |
|---|---|---|---|---|---|---|
| Zn + Pt. | Between | 1 | in | 9,300,000 | and | 9,388,185 |
| Cd + Pt. | ,, | 1 | ,, | 574,000 | ,, | 637,000 |
| Mg + Pt. | ,, | 1 | ,, | 516,666 | ,, | 574,000 |
| Al + Pt. | ,, | 1 | ,, | 12,109 | ,, | 15,000 |

With the Reflecting Galvanometer.

| | | | | | | |
|---|---|---|---|---|---|---|
| Zn + Pt. | Between | 1 | in | 15,500,000 | and | 23,250,000 |
| Cd + Pt. | ,, | 1 | ,, | 1,162,500 | ,, | 1,550,000 |
| Mg + Pt. | ,, | 1 | ,, | 775,000 | ,, | 930,000 |
| Al + Pt. | ,, | 1 | ,, | 42,568 | ,, | 46,500 |

Table V.—Mg + Pt + Iodine in 465 grains of Water at 14° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·132 | 1·5313 rose to 1·777 | 0·0546 | 1·4541 rose to 1·777 |
| 0·119 | ,, ,, ,, | 0·0417 | 1·522 |
| 0·1062 | 1·5112 ,, ,, | 0·0288 | 1·5588 |
| 0·0933 | ,, ,, ,, | 0·0159 | ,, |
| 0·0804 | 1·4741 ,, ,, | 0·003 | ,, |
| 0·0675 | 1·4598 ,, ,, | .. | .. |

ıe electromotive force in the seven strongest solutions rose quickly immersion; this was due to an extremely thin solid coating ng upon the magnesium.

Table VI.—Ditto at 19° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·00099 | 1·7018 | 0·000723 | 1·5588 |
| 0·00089 | 1·7089 | 0·00066 | „ |
| 0·00088 | 1·6589 | 0·00033 | „ |
| 0·000805 | 1·6446 | water | „ |

th the astatic galvanometer the minimum proportion of iodine red to alter the potential lay between 1 in 577,711 and 643,153 of water. If the magnesium was merely wiped between each ırement, instead of being cleaned with emery cloth, the electro-e forces on first immersion were 0·18 volt higher in Tables V I.

› smallest proportion of iodine necessary to upset the balance of ıc-platinum couple in water has already been published luence of the Chemical Energy of Electrolytes, &c.," 'Roy. Proc.,' June 7, 1888); it lay between 1 part in 3,100,000 and 970.

ıle VII.—Cd + Pt + Iodine in 465 grains of Water at 19° C.

| ains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 132 | 0·9884 | 0·0675 | 1·0027 | 0·0030 | 0·8311 |
| ·119 | 0·9741 | 0·0546 | 0·9854 | 0·002625 | 0·8028 |
| 1062 | „ | 0·0417 | 1·0198 | 0·002325 | 0·7882 |
| ·0933 | 0·9884 | 0·0288 | 0·9854 | 0·002079 | 0·747 |
| 0804 | 0·9827 | 0·0159 | 0·9741 | water | „ |

› minimum proportion of iodine required to change the poten-y between 1 part in 200,431 and 224,637 parts of water.

› curves of variation of electromotive force by uniform change ·ength of liquid with zinc-platinum and cadmium-platinum, ıted certain similarities, but that with magnesium-platinum was *erably different,* probably in consequence of insoluble films *g upon the magnesium.*

The following are the proportions of iodine which were required to change the potentials, when the astatic galvanometer was employed:—

Table VIII.

| | | | | | |
|---|---|---|---|---|---|
| Zn + Pt. | Between 1 | part in | 3,100,000 | and | 3,521,970 |
| Mg + Pt. | „ 1 | „ | 577,711 | „ | 643,153 |
| Cd + Pt. | „ 1 | „ | 200,431 | „ | 224,637 |

Table IX.—Mg + Pt + Bromine in 13,950 grains of Water at 12° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·000045 | 1·5757 | 0·00003375 | 1·5600 |
| 0·0000405 | 1·5600 | 0·0000225 | „ |
| 0·000036 | „ | water | „ |

The smallest proportion of bromine required to change the balance lay between 1 part in 310,000,000 and 344,444,444 parts of water.

The minimum proportion necessary to disturb the potential of a zinc-platinum couple in water has been already given ('Roy. Soc. Proc.,' May 31, 1888), and was between 1 part in 77,500,000 and 84,545,000.

Table X.—Cd + Pt + Bromine in 465 grains of Water at 19° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 20·1 | 1·8881 | 13·26 | 1·824 | 6·42 | 1·5163 |
| 18·39 | 1·8709 | 11·55 | 1·5492 | 4·71 | 1·589 |
| 16·68 | 1·8538 | 9·84 | 1·5349 | 3·0 | 1·543 |
| 14·97 | 1·8307 | 8·13 | „ | .. | .. |

The strongest solution was a saturated one.

Table XI.—Ditto at 19° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 3·00 | 1·543 | 1·65 | 1·4174 | 0·3 | 1·2801 |
| 2·85 | „ | 1·5 | „ | 0·15 | 1·2029 |
| 2·7 | 1·5287 | 1·35 | „ | 0·015 | 1·0456 |
| 2·55 | „ | 1·2 | „ | 0·0015 | 0·9084 |
| 2·4 | 1·5258 | 1·05 | „ | 0·00015 | 0·7882 |
| 2·25 | „ | 0·9 | 1·4317 | 0·000134 | 0·7653 |
| 2·1 | 1·5201 | 0·75 | 1·4117 | 0·0001206 | 0·747 |
| 1·95 | „ | 0·6 | 1·3932 | water | „ |
| 1·8 | 1·463 | 0·45 | 1·3173 | .. | .. |

The smallest proportion necessary to disturb the potential lay between 1 in 3,470,112 and 3,875,000. With the solutions from 0·15 to 1·65 grain, the electromotive forces were variable without any apparent cause.

The proportions of bromine required to change the potential with these couples were as follows:—

Table XII.

| | | | | |
|---|---|---|---|---|
| Mg + Pt with bromine. | Between | 1 part in | 310,000,000 | |
| | | and | 344,444,444. | |
| Zn + Pt „ | „ | 1 part in | 77,500,000 | |
| | | and | 84,545,000 | |
| Cd + Pt „ | „ | 1 part in | 3,470,112 | |
| | | and | 3,875,000 | |

The magnitudes of the proportions of bromine required to change the potential with the three couples varied directly as the atomic weights of the three positive metals.

Mg + Pt + Chlorine in 465 grains of Water at 13° C.

Sixteen different solutions, varying in strength from 1·0695 grain to 0·03 grain, with a constant difference of 0·0693 grain, gave each the same potential, viz., 2·7336 volts. Much gas was set free at the magnesium, but only in the stronger solutions. Owing to the extreme sensitiveness of this couple to chlorine, several series of measurements were necessary in order to determine the minimum point with approximate accuracy, and include the entire range of solution.

Table XIII.—Ditto at 13° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·030 | 2·7336 | 0·015 | 2·3906 |
| 0·027 | 2·562 | 0·012 | 2·362 |
| 0·024 | 2·505 | 0·009 | 2·3191 |
| 0·021 | 2·4478 | 0·006 | 1·9546 |
| 0·018 | 2·4192 | 0·003 | 1·9118 |

Table XIV.—Ditto at 13° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·003 | 1·9117 | 0·0015 | 1·9117 |
| 0·0027 | „ | 0·0012 | „ |
| 0·0024 | „ | 0·0009 | „ |
| 0·0021 | „ | 0·0006 | „ |
| 0·0018 | „ | 0·0003 | „ |

Table XV.—Ditto at 13° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·0003 | 1·9117 | 0·00000117 | 1·782 |
| 0·00015 | „ | 0·00000058 | 1·7620 |
| 0·000075 | „ | 0·00000029 | 1·7248 |
| 0·0000375 | „ | 0·000000145 | 1·6819 |
| 0·00001875 | 1·8249 | 0·0000000725 | 1·639 |
| 0·00000937 | 1·8106 | 0·000000036 | 1·6047 |
| 0·00000468 | 1·7992 | 0·000000018 | 1·5589 |
| 0·00000234 | 1·7906 | water | „ |

Table XVI.—Ditto in 13,950 grains of Water at 12·5° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·000000891 | 1·573 | 0·000000713 | 1·5589 |
| 0·000000821 | „ | 0·0000003565 | „ |
| 0·000000792 | 1·5589 | water | „ |

*In this table, the delicacy of the thermo-pile was increased reducing the difference of temperature between its junctions fr 100 Centigrade degrees to 50.*

With the astatic galvanometer, the electromotive force of the )uple in water began to change when the proportion of chlorine was stween 1 part in 17,000 million and 17,612 million parts of water; ıt with the reflecting one it was between 1 in 29,062 millions and ¦,291 millions.

The minimum proportion of chlorine required to change the potenal of a zinc-platinum couple, when the astatic galvanometer was nployed, lay between 1 part in 1,264 millions and 1,300 million parts : water ("Influence of the Chemical Energy of Electrolytes, &c.," Roy. Soc. Proc.,' June 7, 1888).

Table XVII.—Cd + Pt + Chlorine in 465 grains of Water at 19° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 1·0695 | 1·71654 | 0·6537 | 1·7339 | 0·2379 | 1·7137 |
| 1·0002 | 1·730 | 0·5844 | 1·7251 | 0·1686 | 1·7022 |
| 0·9309 | 1·7683 | 0·5151 | 1·7223 | 0·0993 | 1·6856 |
| 0·8616 | 1·7453 | 0·4458 | 1·7165 | 0·03 | 1·6062 |
| 0·7923 | 1·739 | 0·3765 | 1·7022 | .. | .. |
| 0·723 | " | 0·3072 | 1·6885 | .. | .. |

Table XVIII.—Ditto at 19° C.

| Grains. | Volts. | Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|---|---|
| 0·03 | 1·6062 | 0·015 | 1·5175 | 0·0003 | 1·1028 |
| 0·027 | " | 0·012 | 1·4889 | 0·00010695 | 0·7904 |
| 0·024 | " | 0·009 | 1·4603 | 0·00005346 | 0·7589 |
| 0·021 | 1·5690 | 0·006 | 1·4346 | 0·00004806 | 0·7475 |
| 0·018 | 1·5575 | 0·003 | 1·3459 | water | " |

The smallest proportion of chlorine necessary to change the potential lay between 1 part in 8,773,585 and 9,270,833 parts of water.

The following results were obtained by varying the kind of negative metal:—

Table XIX.—Zn + Au + Chlorine in 13,950 grains of Water at 15° C.

| Grains. | Volts. |
|---|---|
| 0·000026928 | 1·0371 |
| 0·000025344 | 1·0228 |
| 0·000024947 | " |
| water | " |

The minimum proportion of chlorine in this case lay between 1 in 518,587,360 and 550,513,022 parts of water.

Table XX.—Zn + Cd + Chlorine in 1550 grains of Water at 11° C.

| Grains. | Volts. | Grains. | Volts. |
|---|---|---|---|
| 0·3565 | 0·2687 | 0·02027 | 0·32032 |
| 0·05592 | 0·2831 | water | „ |
| 0·02796 | 0·3088 | .. | .. |

Eleven other solutions of different strengths, all weaker than 0·02027, each gave the same potential as water. The minimum proportion of chlorine required to disturb the balance lay between 1 part in 55,436 and 76,467 parts of water. In this case, the addition of chlorine decreased the electromotive force; a similar effect occurred with a zinc-platinum couple in a solution of potassic iodide ("Influence of the Chemical Energy of Electrolytes, &c.," 'Roy. Soc. Proc.,' June 7, 1888).

The following are the minimum proportions of chlorine which were required to change the potential:—

Table XXI.

With an Astatic Galvanometer.

| | | | | | |
|---|---|---|---|---|---|
| Mg + Pt + Cl. | Between | 1 in | 17,000,000,000 | and | 17,612,000,000 |
| Zn + Pt + Cl. | „ | 1 „ | 1,264,000,000 | „ | 1,300,000,000 |
| Zn + Au + Cl. | „ | 1 „ | 518,587,360 | „ | 550,513,022 |
| Cd + Pt + Cl. | „ | 1 „ | 8,733,585 | „ | 9,270,833 |
| Zn + Cd + Cl. | „ | 1 „ | 55,436 | „ | 76,467 |

With a Reflecting Galvanometer.

Mg + Pt + Cl. Between 1 in 27,062 millions and 32,291 millions.

The examples contained in this paper are sufficient to show, that the proportion of the same exciting liquid, necessary to disturb the potential of a voltaic couple in water, and the order of variation of potential caused by change of strength of liquid, vary with each different positive or negative* metal. The numbers in Tables IV, VIII, XII and XXI, show that the more positive or more easily corroded the positive metal, or the more negative and less easily corroded the negative one, the smaller usually was the proportion of dissolved substance required to change the potential. In the case of chlorine,

* *If the negative metal is not at all corroded, the order of change of potential by change of negative metal is not much affected.*

well as in that of bromine, the magnitudes of the minimum pro- rtions of substance necessary to change the potential of magnesium- atinum, zinc-platinum, and cadmium-platinum couples, varied rectly as the atomic weights of the positive metals.

The experiments also show that the degree of sensitiveness of the rangement for detecting the minimum-point of change of poten- al depends largely upon the kind of galvanometer employed. As a ore sensitive galvanometer enables us to detect a change of potential used by a much smaller proportion of material; and as the propor- on of substance capable of detection is smaller the greater the free emical energy of each of the uniting bodies, it is probable that the ectromotive force really begins to increase with the very smallest ldition of the substance, and might be detected if our means of tection were sufficiently sensitive or the free chemical energy was fficiently strong.

## II. "Magnetic Qualities of Nickel (Supplementary Paper)." By J. A. Ewing, F.R.S., Professor of Engineering in University College, Dundee. Received June 14, 1888.

(Abstract.)

The paper is a supplement to one with the same title by Professor ring and Mr. G. C. Cowan, which was read at a recent meeting of e Society. It describes experiments, conducted under the author's ection by two of his students, Mr. W. Low and Mr. D. Low, on the ects of longitudinal compression on the magnetic permeability and entiveness of nickel. The results are exhibited by means of curves, wing the relation which was determined between the intensity of gnetisation of the metal and the magnetising force, when a nickel , reduced to approximate endlessness by a massive iron yoke which med a magnetic connexion between its ends, was magnetised under re or less stress of longitudinal compression. Corresponding curves w the relation of residual magnetism to magnetising force, for ious amounts of stress; and others are drawn to show the relation nagnetic permeability to magnetic induction. Initial values of the meability, under very feeble magnetising forces, were also deter- ied. The experiments were concluded by an examination of the aviour of nickel in magnetic fields of great strength. Magnetising es ranging from 3000 to 13,000 c.g.s. units were applied by cing a short bobbin with a narrow neck made of nickel between the es of a large electromagnet, and it was found that these produced a ctically constant intensity of magnetisation which is to be accepted the *saturation value.*

## VIII. "Evaporation and Dissociation. Part VIII. A Study of the Thermal Properties of Propyl Alcohol." By WILLIAM RAMSAY, Ph.D., F.R.S., and SYDNEY YOUNG, D.Sc. Received June 14, 1888.

(Abstract.)

In continuation of our investigations of the thermal properties of pure liquids, we have now determined the vapour-pressures, vapour-densities, and expansion in the liquid and gaseous states of propyl alcohol, and from these results we have calculated the heats of vaporisation at definite temperatures. The compressibility of the liquid has also been measured. The range of temperature is from 5° to 280° C., and the range of pressure from 5 mm. to 56,000 mm.

The memoir contains an account of the purification of the propyl alcohol; determinations of its specific gravity at 0°, and at 10°·72; and of the constants mentioned above.

The approximate critical temperature of propyl alcohol is 263°·7; the approximate critical pressure is 38,120 mm., and the approximate volume of one gram is 3·6 c.c. The first two of these constants must be very nearly correct; the third cannot be determined with the same degree of precision.

The memoir is accompanied by plates, showing the relations of volume, temperature, and pressure in a graphic form.

## IX. "Contributions to the Chemistry of Chlorophyll. No. III." By EDWARD SCHUNCK, F.R.S. Received June 19, 1888.

(Abstract.)

This paper is a continuation of the previous ones on the same subject. In it the author gives an account of the action of alkalis on phyllocyanin so far as regards the first stage of the process, and of the products thereby formed. Phyllocyanin when acted upon by alkalis yields in the first instance a well-crystallised substance of a peacock- or steel-blue colour, to which he gives the name of *Phyllotaonin*. He describes its properties and those of some of its compounds. When hydrochloric acid gas in excess is passed through a solution of chlorophyll in alcoholic soda, a compound crystallising in lustrous purple needles is formed, which seems to be the ethyl ether of phyllotaonin. By substituting methylic for ethylic alcohol a very similar compound is obtained, which the author considers to be the corresponding methyl ether. Though these compounds readily yield phyllotaonin by saponification with alcoholic potash or soda, the author did not succeed in reproducing them by the combined action of alcohol and hydrochloric acid on phyllotaonin.

X. "On the Specific Resistance of Mercury." By R. T. GLAZEBROOK, M.A., F.R.S., Fellow of Trinity College, and T. C. FITZPATRICK, B.A., Fellow of Christ's College, Demonstrators in the Cavendish Laboratory, Cambridge. Received June 19, 1888.

(Abstract.)

The paper contains an account of experiments made to determine the value of the resistance of a column of mercury, 1 metre long and 1 sq. mm. in cross section, in terms of the B.A. unit. The method employed differed very slightly from that of Lord Rayleigh and Mrs. Sidgwick ('Phil. Trans.,' 1883). Tubes of about 1, 2, and 3 sq. mm. in cross section were calibrated and filled with mercury. They were then immersed in melting ice, and their resistance compared with that of the B.A. standards, using Carey Foster's method and the B.A. bridge. The length of the mercury column, occupying nearly the whole of the tube, was measured, and the mass of the same determined. From this the average cross section is obtained, and hence the value of $r$, the resistance of a column 1 metre long, 1 sq. mm. in cross section. The mercury used to find the cross section was with few exceptions that which had been employed in finding the resistance. The results of the measurements are given in Table I.

In the table, Column 1 gives the number of the tube, Column 2 the number of the observation. L is the length of the tube, and $a$ the mean radius of the cross section, R the observed resistance in B.A. units. The mean value of $r$ found from the three 1 mm. tubes is 0·95354 B.A. units. The other four tubes of one-half and one-third units respectively lead to the value $r = 0·95344$ B.A. units. The difference between the two is considerable, and reasons are given for assigning more weight to the first value.

For an account of the experiments and of the small precautions necessary to secure accuracy, reference must be made to the paper.

Table II gives a list of the various values which have been found for $r$ with the lengths of the column of mercury which, according to the different observers, has a resistance of 1 ohm ($10^9$ C.G.S. units of resistance). In combining our own observations we have assigned weights to the various tubes inversely proportional to their diameters, and we find as our final value

$$r = 0·95352.$$

Table I.

| No. of Tube. | | L. | a. | R. | r. | Mean value of r from each tube. |
|---|---|---|---|---|---|---|
| VI. | 1. | 113·134 | 0·0586 | 1·000010 | 0·95358 | 0·95357 |
| | 2. | ,, | ,, | 0·999949 | 0·95364 | |
| | 3. | ,, | ,, | 0·999949 | 0·95353 | |
| | 5. | ,, | ,, | 0·999915 | 0·95351 | |
| | 6. | ,, | ,, | 0·999982 | 0·95357 | |
| | 8. | ,, | ,, | 0·999955 | 0·95360 | |
| VIII. | 1. | 127·438 | 0·0622 | 1·000133 | 0·95351 | 0·95354 |
| | 2. | ,, | ,, | 1·000103 | 0·95349 | |
| | 3. | ,, | ,, | 0·999996 | 0·95353 | |
| | 5. | ,, | ,, | 1·000040 | 0·95357 | |
| | 7. | ,, | ,, | 1·000036 | 0·95361 | |
| II. | 1. | 101·904 | 0·0553 | 1·011926 | 0·95353 | 0·95349 |
| | 2. | ,, | ,, | 1·011805 | 0·95340 | |
| | 3. | ,, | ,, | 1·011810 | 0·95356 | |
| | 4. | ,, | ,, | 1·011803 | 0·95346 | |
| V. | 1. | 112·950 | 0·0829 | 0·499233 | 0·95334 | 0·95338 |
| | 2. | ,, | ,, | 0·499215 | 0·95342 | |
| IX. | 1. | 110·913 | 0·0820 | 0·500388 | 0·95344 | 0·95344 |
| | 2. | ,, | ,, | 0·500333 | 0·95343 | |
| I. | 1. | 91·723 | 0·0915 | 0·332535 | 0·95343 | 0·95344 |
| | 3. | ,, | ,, | 0·332574 | 0·95346 | |
| III. | 1. | 101·765 | 0·0968 | 0·329854 | 0·95345 | 0·95351 |
| | 2. | ,, | ,, | 0·329867 | 0·95357 | |

Table II.

| Observer. | Date. | Value for $r$ in B.A. units. | Value of ohm in centimetres of mercury at 0°. |
|---|---|---|---|
| Lord Rayleigh and Mrs. Sidgwick .... | 1883 | 0·95412 | 106·23 |
| Mascart, Nerville, and Benoit ........ | 1884 | 0·95374 | 106·33 |
| Strecker........................ | 1885 | 0·95334 | .. |
| L. Lorentz ...................... | 1885 | 0·95388 | 105·93 |
| Rowland ......................... | 1887 | 0·95349 | 106·32 |
| Kohlrausch ...................... | 1888 | 0·95331 | 106·32 |
| Glazebrook and Fitzpatrick......... | 1888 | 0·95352 | 106·29 |

The paper contains a discussion of the above results. It is shown hat probably Lord Rayleigh's value of $r$ may be too high by as much s 0·0002, in consequence of the fact that the mercury in his terminal ups was 5° or 6° C., but no complete explanation of the differences etween his result and those of Rowland, Kohlrausch, and ourselves, as been found. The difficulty of working with tubes such as those sed by the Lorentz, 1—2 metres in length, and 1, 2, and 3 cm. in iameter, may perhaps account for his value for the ohm, viz., 105·93.

## I. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. VI. On the Anomodont Reptilia and their Allies." By H. G. SEELEY, F.R.S. Received June 20, 1888.

(Abstract.)

The author examines the structure of the skull in the Dicynodontia, d discusses the interpretations of its elements and affinities given Sir Richard Owen, Professor Huxley, and Professor Cope, and rives at the conclusion that the interpretation of the bones of the late may be varied. The quadrate bone is found, though it is sent from many specimens owing to loose articulation, and the lleus is recognised as a normal element in the skull, which ticulates with the quadrate and is free, except at its extremities. e palatine bones are internal to the pterygoids, and the pteryids extend forward to the maxillary. The columella is found more than one specimen. Many new specimens are described ich *further elucidate* the structure of the skull. The first of se *shows that the* upper part of the foramen magnum is formed

by the supra-occipital bone, and that the element which has appeared to be a supra-occipital is the inter-parietal. Evidence is given of the form of the brain case, which is found to be high and narrow. Details are given of the structure of the squamosal bone, and of its relation to the quadrate and other cranial elements; and it appears that the squamosal usually embraces the quadrate, so as to extend in front of it, and sometimes to hide it, so that both the quadrate and squamosal sometimes contribute to form the articulation for the lower jaw. Evidence is offered of the sutures which divide the bones of the skull from each other. The sub-nasal element, found in *Pareiasaurus*, is met with in Dicynodonts, sometimes below the narine, and sometimes within its floor in the position of a turbinal. A new type of quadrate bone, which is regarded as Anomodont, is described, and found to differ from the usual form in being perforated in the antero-posterior direction. A summary of the structure of the skull is illustrated by a restoration showing its sutures.

Further contributions are made to a knowledge of the vertebral column. The cervical vertebræ are described, the atlas and axis are regarded as anchylosed, and succeeded by an intercentrum which has no neural arch. The cervical ribs are comparatively long, and articulate by a long fork with the neural arch, as well as with the centrum. Further evidence is given of the structure of dorsal vertebræ, showing that the rib is attached to a single transverse process of the neural arch. The caudal vertebræ of *Platypodosaurus*, eleven in number as preserved, are described; and some observations are made on the mode of ossification of the intervertebral substance. Additional materials further elucidate the Anomodont scapular arch, and examples of scapula and coracoid are described; but the only additional pelvic bone described is the pubis of *Titanosuchus*.

An account is given of the limb bones, which are elucidated by large bones associated with the skull fragments described by Sir R. Owen as *Titanosuchus ferox*. They contribute to a knowledge of the femur, humerus, and fibula in that type, and are associated with small bones of the extremities which are probably metacarpals. The ulna is described, which was referred by Sir R. Owen to *Pareiasaurus*, and evidence is given that it possessed terminal epiphyses of different form to any which are known in fossil reptiles, the proximal epiphysis having much the character of the olecranon of a mammal. A massive Anomodont tibia, also referred by Sir R. Owen to *Pareiasaurus*, is described, and found to possess a distal talon of mammalian pattern.

Further observations are made upon the Theriodontia, as restricted to the genus *Galesaurus*, the skull of which is further elucidated. The author also describes new material, making known the structure of the skull, palate, and scapular arch of Procolophon; from which it appears that the pre-coracoid is exceptionally well developed, and

united by suture to the coracoid. The inter-clavicle had the slender T-shaped form of the bone in *Ichthyosaurus*.

*Procolophon* has teeth on the vomera and pterygoid bones, and the structure of the palate and the post-orbital region show that the Procolophonia forms a distinct division of the Anomodontia. Observations are made on the relations of the European and South African Anomodonts, and on the relation of the Anomodontia to the Pelycosauria and to Cotylosauria. Comparison is made with *Placodus*, which genus has two exoccipital condyles, comparable to those of mammals, and appears to have lost the basi-occipital condyle. Comparisons are made with other extinct reptilia to show the relation of the Anomodonts to the Saurischia, and other reptilian types. Observations are offered on the theory of the Anomodont skull, and on the effect of the articulation of the lower jaw with the squamosal in causing a diminished growth of the malleus and quadrate, converting them into the malleus and tympanic.

The larger groups included in the Anomodont alliance are regarded as the Pareiasauria and Procolophonia; Dicynodontia, Gennetotheria, and Pelycosauria; the Theriodontia, Cotylosauria, and Placodontia are regarded as coming under the same sub-class, which at one end of the series exhibits characters which link reptiles with amphibians, and at the other end of the series link reptiles with mammals.

## XII. "A new Form of Eudiometer." By William Marcet, M.D., F.R.S. Received June 20, 1888.

[Plate 14.]

The quantitative determination of oxygen, simple as it appears at first sight, is found in practice beset with many difficulties. Liebig's method with pyrogallic acid and potassium hydrate, though considered as yielding correct results, takes too much time, and is unsatisfactory in some respects, so that the eudiometer has become of general use for the estimation of oxygen. I shall not attempt to describe the various forms of eudiometer, but it may be assumed that Regnault, so well known for the care he bestowed on his investigations, had adopted a very correct kind of eudiometer in the researches he undertook with Reiset on the chemical phenomena of respiration.* Other eudiometers have been made since then, such as the ingenious instrument of Dr. Frankland for gas analysis, which has proved most serviceable. I claim for the present form of eudiometer that it is correct and reliable in its working, simple in construction, and easy of manipulation. The main objects of an eudiometer must be the easy introduction of the air to be analysed, the ready mixture of that air with a known volume of pure hydrogen gas, and the correct reading

* '*Annales de Chimie et de Physique*,' 3rd Series, vol. 26, 1849.

of the volume after explosion. It will be seen that these conditions are entirely fulfilled in the present instrument; and it has, moreover, the advantage of being available in conjunction with Pettenkofer's method for the determination of carbonic acid in atmospheric air.

The eudiometer as figured in the accompanying Plate has the form of a T-piece, the vertical limb of which is a straight tube about 60 cm. in length and 12 cm. in diameter; it is divided into 50 or 60 c.c. and tenths of c.c., like a common burette. The upper end of this tube is closed air-tight with a steel cap, from which lateral tubes project right and left; these tubes are bent V-shaped, or rather in the form of a lyre. At the junction of the lateral tubes with the cap, there is a three-way stop-cock allowing of the passage of air or gas in four different directions, viz., first through the tubes cut off from the body of the eudiometer; secondly, into the eudiometer, which is done by raising it in the mercury trough; thirdly, out of the eudiometer, on the side opposite that from which it was introduced, which is effected by depressing the tube in the mercury; fourthly, through the tubes and eudiometer simultaneously. The eudiometer is held tightly by two claws projecting at different heights from a vertical iron rod connected with a rack and pinion movement. The iron rod, together with the eudiometer, is immersed in mercury contained in a straight cylindrical glass vessel.

The hydrogen used for the explosion is prepared for that special object from zinc and sulphuric acid in the ordinary way, and washed through an alkaline solution, rather than obtained condensed in iron bottles from the manufacturers, and it is collected in a bell-jar suspended over water. The bell-jar I use holds 11 litres of gas; it is balanced by a counterpoise, and its weight, as it moves up and down in water, is regulated by another counterpoise hanging from a cycloid, so that the gas in the holder is always under atmospheric pressure; an oil-gauge fixed to the holder shows at any time the pressure in the bell-jar. Should the gas fail to be absolutely under atmospheric pressure, the equality of pressures may be ensured by the use of the adjusting instrument I have described in a former communication. It consists of a clamp fixed to the rim of the tank, and made to grasp at will the cord holding the counterpoise; a screw in connexion with the clamp enables the cord, and consequently the bell-jar, to be drawn up or down. For the actual requirements of the analysis, a receiver for the hydrogen holding only one litre of gas would suffice, but it is better to have a larger gas-holder in which to store up the hydrogen for future determinations.

Moreover, the cycloid arrangement for regulating the weight of the bell-jar, though very convenient, may be dispensed with, as the gas in the receiver can be brought approximately under atmospheric pressure

ıy means of weights, while the adjusting screw will enable its being .ccurately placed under atmospheric pressure.

The analysis is made as follows:—

We suppose that air for analysis has been shaken with barium ıydrate in a glass jar of a capacity of about 10 litres, and made ccording to the form adopted by Dr. Angus Smith* for the letermination of carbonic acid in air by Pettenkofer's method. This ar is closed by a tight-fitting india-rubber cap, which I cover with everal coats of copal varnish; from this cap two short india-rubber ubes project, each of these tubes being clamped by a pinch-cock. lfter the agitation is over, and when all the carbonic acid is taken .p by the alkaline solution, the fluid is poured out from the jar into glass-stoppered bottle holding about 100 c.c. This can be done asily without letting any air into the jar, as the india-rubber cap ʳill collapse somewhat while the fluid is allowed to run out through ne of the india-rubber tubes in the cap, a very small quantity of uid only being left in the jar. The india-rubber tube is again amped, and the bottle holding the barium hydrate is sealed with araffine and left undisturbed for the precipitation of the carbonate ıd subsequent analysis.

The glass jar full of air free from carbonic acid, and absolutely ıturated with moisture, is placed under a funnel supported on a lter stand, and the funnel is connected with one of the india-rubber ıbes projecting from the cap, while the other tube has a short piece f glass tubing inserted into it, to which a longer india-rubber tube is xed.

Everything is now ready for the determination of the oxygen of the ir contained in the glass jar. After turning the stop-cock in the cap f the eudiometer, so as to allow the hydrogen gas to wash out the teel tubes and top of the eudiometer, the latter is lowered in the ylinder until the mercury is in contact with the cap, and therefore ery near to the stop-cock. The eudiometer is next connected by arrow india-rubber tubing with the hydrogen receiver on which a ʳeight has been placed, and on opening the receiver hydrogen rushes ut, washing thoroughly the passage through which it will have to each the eudiometer, and driving out the very small quantity of air ontained in the steel cap between the mercury and the stop-cock. I ound it convenient to stop the end of the V-shaped tube letting out be gas with short india-rubber tubing and a pinch-cock. When a few undred cubic centimetres of gas have gone through, the three-way ıp is turned by one-quarter of a turn, so as to place the tube in ommunication with the hydrogen; it is now easy to rinse the eudioıeter with that gas, by raising the eudiometer, and then giving the aree-way cock half a turn, so as to bring the instrument in communi-

* 'Air and Rain,' 1872.

cation with the external air; the endiometer is then rapidly depressed and closed. In this position the tube from the hydrogen can be rinsed again, independently of the eudiometer, so that the washing may be considered as complete and thorough.

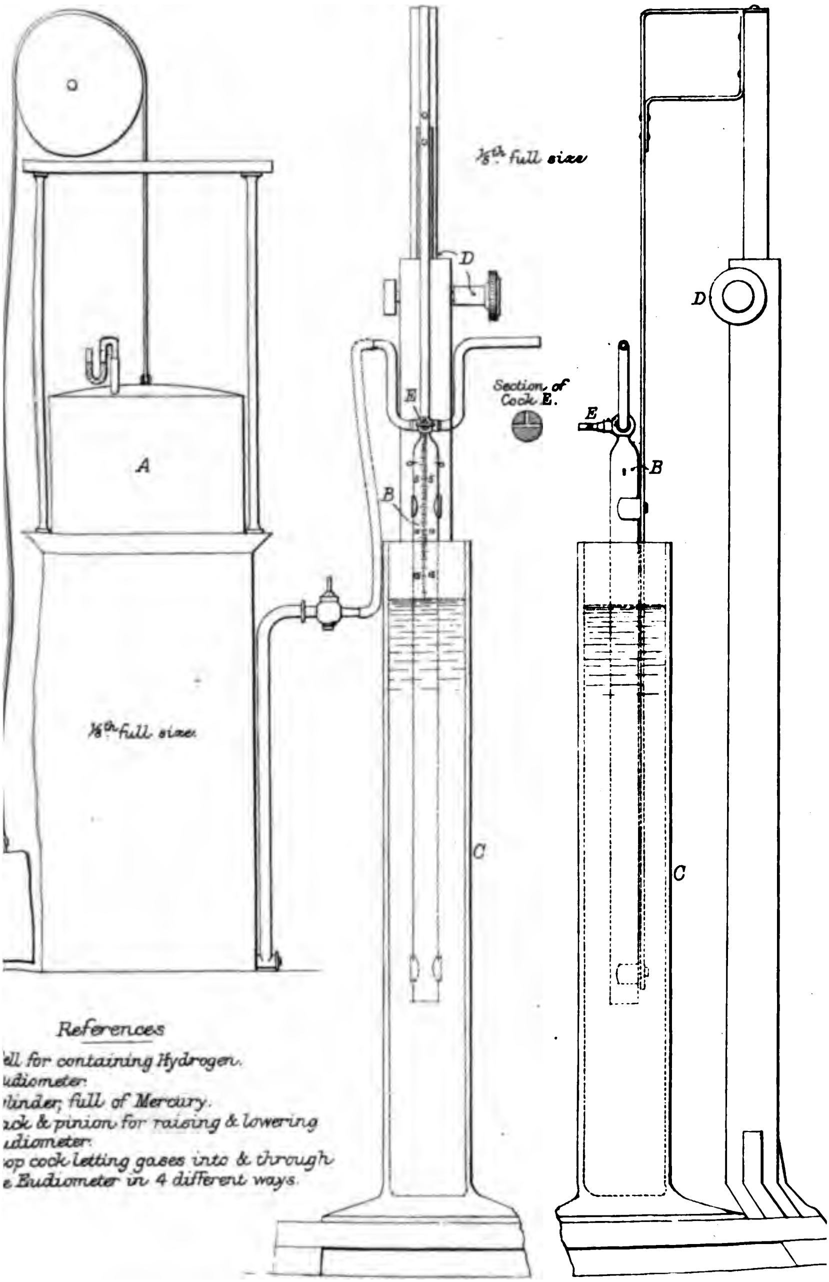
The eudiometer being brought into connexion with the hydrogen is again raised, and 18 c.c. of hydrogen gas are taken in under atmospheric pressure.

The hydrogen kept over water is saturated, and a thermometer with its bulb in the bell-jar gives the temperature of the gas, which is very nearly that of the laboratory; so that by the time the gas is ready to be measured in the eudiometer it shows no tendency either to contract or dilate. The eudiometer now contains the volume of hydrogen required for the analysis, and the stop-cock is turned shutting off the gas from the holder, and opening the V-shaped tubes through and through in readiness for washing out with the air to be analysed.

The air from the large glass jar is introduced into the eudiometer in the following way. Having filled the funnel referred to above with water, the latter is let into the jar by opening slightly the pinch-cock closing the funnel; at the same time the glass jar having been connected with the V-shaped tube of the eudiometer by india-rubber tubing, is opened towards the instrument, when the air displaced by the water added rinses out the india-rubber and steel tubings. There is plenty of air in the jar, so that no necessity occurs to be saving; when the tubes are rinsed the eudiometer is raised in the mercury up to about 45 c.c., carrying a column of mercury with it; then the two-way stop-cock is very carefully turned so as to admit the air to be analysed, which is aspired by the mercury as it subsides. Thus some 27 c.c. of air are introduced. The aspiration must be fairly rapid, and the fall of mercury in the tube should be stopped by turning the stop-cock before the mercury has quite reached its level in the trough, otherwise there is a risk of a recoil of the mercury, and a "pumping" which it is important to avoid. The mixed gases are left undisturbed for two or three minutes, and their volume is read off under atmospheric pressure, the eudiometer being next moved up and down in the mercury by a few centimetres, so as to effect the perfect mixture of the gases. The instrument is now slightly raised, carrying with it a short column of mercury, and the gases are ignited by the electric spark under reduced atmospheric pressure. This mode of proceeding, recommended by McLeod,* weakens considerably the violence of the explosion, and ensures perfect safety. Immediately after the explosion the gas in the eudiometer is brought approximately under atmospheric pressure.

* *McLeod*, "On a new Form of Apparatus for Gas Analysis," 'Chem. Soc. *Journ.*,' *1869.*

Front Elevation. End Elevⁿ




References

[B]ell for containing Hydrogen.
[E]udiometer.
[Cy]linder, full of Mercury.
[R]ack & pinion for raising & lowering [E]udiometer.
[St]op cock letting gases into & through [th]e Eudiometer in 4 different ways.

ᴡ contraction now takes place as the heat produced by the ı is radiated from the instrument; it is advisable to wait ᴠenty minutes, until the contraction is complete, and the ıf the gas is read off under atmospheric pressure.

strument should be sheltered from any draught, or from the diation of a fire, and indeed be kept from any change of ure, and with that object I find it advisable to shelter it with ırd tubular shield sliding up and down the mercury trough.

taken directly from the atmosphere is to be analysed, in order ɔ its being saturated it will be advisable to pass it through a of wet horse-hair, and obtain it directly from the tube into ɔmeter. In the above account of the manipulation required, ·ogen is introduced before the air into the eudiometer. I ·d to let in the air first, but this plan was not successful ly because the mixture of air and hydrogen was incomplete ıe explosion. The hydrogen being collected first in the ·er will rise from its comparative lightness as the air is ı and mix with it perfectly, while the stream is sufficiently prevent any of the mixture from diffusing out of the tube. . be borne in mind that after a number of analyses the water from the explosions accumulates on the surface of the in the eudiometer, and the mercury meniscus is no longer een. This can be easily avoided by drying the tube with paper after a certain number of analyses. The following are terminations of oxygen in atmospheric air made with the eudiometer described above. They are not selected, but succession in the order in which they were made. And I e beg to record the valuable aid of my assistant, Mr. Charles ıend, F.C.S., in the present inquiry.

Oxygen per cent. in Atmospheric Air.

| First Series. | Second Series.* |
|---|---|
| 21·01 | 20·94 |
| 20·98 | 20·93 |
| 21·00 | 20·96 |
| 20·97 | 20·95 |
| 20·97 | 20·93 |
| ——— | 20·95 |
| ean .. 20·99 | 20·96 |
| difference, 0·2 per cent. | ——— |
| | Mean .. 20·946 |
| | Greatest difference, 0·14 per cent. |

* *ıe analysis omitted:* obviously too high from insufficient rinsing.

## XIII. "Theorems in Analytical Geometry." By W. H. L. Russell, F.R.S. Received June 21, 1888.

To determine the envelope of the first polar of any curve, when the pole moves on a given curve of the third order.

Let F $(\xi, \eta, \zeta) = 0$ be equation to the surface; then if $p = \frac{dF}{d\xi}$, $q = \frac{dF}{d\eta}$, $r = \frac{dF}{d\zeta}$, $px + qy + rz = 0$ is the equation to first polar, when $(x, y, z)$ moves on a given cubic,

$$x^3 + y^3 + z^3 + 6mxyz = 0.$$

Then differentiating

$$(x^2 + 2myz)\, dx + (y^2 + 2mxz)\, dy + (z^2 + 2mxy)\, dz = 0.$$

$$pdx + qdy + rdz = 0.$$

Then as usual

$$x^2 + 2myz = \lambda p,$$

$$y^2 + 2mxz = \lambda q,$$

$$z^2 + 2mxy = \lambda r.$$

Then eliminating $z$ by the equation to the first polar, we have—

$$\mathrm{A}x^2 + \mathrm{B}xy + \mathrm{C}y^2 = p,$$

$$\mathrm{D}x^2 + \mathrm{E}xy + \mathrm{F}y^2 = q,$$

$$\mathrm{G}x^2 + \mathrm{H}xy + \mathrm{K}y^2 = r,$$

where A, B, C . . . . are functions of $pqr$, whose forms are immediately seen, and the arbitrary multiplier is omitted because it will disappear in the final result: then we find at once the values of $x^2$, $xy$ and so $z^2$, $y^2$, and therefore of $x$, $y$, $z$, which we may substitute in the equation to the polar, and so obtain the envelope. But we may find a more symmetrical result thus: eliminating as before by means of the equation to the polar—

$$\mathrm{A}'y^2 + \mathrm{B}'yz + \mathrm{C}'z^2 = p,$$

$$\mathrm{D}'y^2 + \mathrm{E}'yz + \mathrm{F}'z^2 = q,$$

$$\mathrm{G}'y^2 + \mathrm{H}'yz + \mathrm{K}'z^2 = r;$$

*and moreover*

$$\mathrm{A}''z^2 + \mathrm{B}''xz + \mathrm{C}''x^2 = p,$$

$$\mathrm{D}''z^2 + \mathrm{E}''xz + \mathrm{F}''x^2 = q,$$

$$\mathrm{G}''z^2 + \mathrm{H}''xz + \mathrm{K}''x^2 = r.$$

:e the equation to the envelope is—

$$= p\left\{\frac{p(\mathrm{EK}-\mathrm{HF}) + q(\mathrm{HC}-\mathrm{BK}) + r(\mathrm{BF}-\mathrm{EC})}{\mathrm{A}(\mathrm{EA}-\mathrm{HF}) + \mathrm{D}(\mathrm{HC}-\mathrm{BK}) + \mathrm{G}(\mathrm{BF}-\mathrm{EC})}\right\}^{\frac{1}{2}}$$

$$q\left\{\frac{p(\mathrm{E'K'}-\mathrm{H'F'}) + q(\mathrm{H'C'}-\mathrm{B'K'}) + r(\mathrm{B'F}-\mathrm{E'C'})}{\mathrm{A'}(\mathrm{E'K'}-\mathrm{H'F'}) + \mathrm{D'}(\mathrm{HC'}-\mathrm{B'K'}) + \mathrm{G'}(\mathrm{B'F'}-\mathrm{E'C'})}\right\}^{\frac{1}{2}}$$

$$\left\{\frac{p(\mathrm{E''K''}-\mathrm{H''F''}) + q(\mathrm{H''C''}-\mathrm{B''K''}) + r(\mathrm{B''F''}-\mathrm{E''C''})}{\mathrm{A''}(\mathrm{E''K''}-\mathrm{H''F''}) + \mathrm{D''}(\mathrm{H''C''}-\mathrm{B''K''}) + \mathrm{G''}(\mathrm{B''F''}-\mathrm{E''C''})}\right\}^{\frac{1}{2}}.$$

hen the curve F is of the third order the first polar becomes a
: of the second order, which is called the polar conic. Let us see
curve the pole must move on for the polar conic to break up into
straight lines. Let—

$$\xi\,(x^2 + 2myz) + \eta\,(y^2 + 2mxz) + \zeta\,(z^2 + 2mxy) = 0,$$

e equation to the polar conic. Then

$$x^2 + 2mx\left(\frac{\eta z}{\xi} + \frac{\zeta y}{\xi}\right) + \left(\frac{\eta}{\xi}y^2 + 2myz + \frac{\zeta}{\xi}z^2\right) = 0,$$

hat this equation may break up into factors

$$m^2\left(\frac{\eta z}{\xi} + \frac{\zeta y}{\xi}\right)^2 - \left(\frac{\eta}{\xi}\,y^2 + 2myz + \frac{\zeta}{\xi}\,z^2\right)^2$$

be a square; or

$$\left(\frac{m^2\eta^2}{\xi^2} - \frac{\zeta\xi}{\xi^2}\right)z^2 + 2m\left(\frac{\zeta\eta m}{\xi^2} - 1\right)yz + \left(\frac{m^2\zeta^2}{\xi^2} - \frac{\eta\xi}{\xi^2}\right)y^2$$

be a square; or

$$\left(\frac{m^2\eta^2}{\xi^2} - \frac{\zeta\xi}{\xi^2}\right)\left(\frac{m^2\zeta^2}{\xi^2} - \frac{\eta\xi}{\xi^2}\right) = m^2\left(\frac{\eta\zeta m}{\xi^2} - 1\right)^2,$$

$$- m^2(\xi^3 + \eta^3 + \zeta^3) + (1 + 2m^3)\xi\eta\zeta = 0,$$

quation to the Hessian.

ence the equation to the straight lines is of the form—

$$x + m\left(\frac{\eta z}{\xi} + \frac{\zeta y}{\xi}\right) = \pm\, m\mathrm{Q},$$

and therefore the line

$$\xi x + m\eta z + m\zeta y = 0$$

must pass through the point of their intersection. So also must

$$\eta y + m\zeta x + mz\xi = 0,$$

$$\zeta z + m\eta x + m\xi y = 0.$$

The pole and the intersection of these two straight lines are called by Dr. Salmon corresponding points. When I had proceeded thus far, and had begun to make deductions from these equations, I became acquainted with the existence of a memoir by Professor Cayley on this subject in the 'Phil. Trans.' for 1857. He has there given these equations without proof. I have therefore demonstrated them exactly in the way in which I discovered them before I was acquainted with his paper, to which I refer for ulterior theorems.

To determine the double tangents of a quartic.

Let $y = mx + a$ be the equation to a straight line cutting the quartic. If this value of $y$ be substituted in the quartic, the equation will become

$$x^4 - Px^3 + Qx^2 - Rx + S = 0,$$

so that if $\alpha$, $\beta$, $\gamma$, $\delta$ be the roots of this equation, we have the following equations:—

$$\alpha + \beta + \gamma + \delta = P,$$

$$\alpha\beta + \alpha\gamma + \alpha\delta + \beta\gamma + \beta\delta + \gamma\delta = Q,$$

$$\alpha\beta\gamma + \alpha\gamma\delta + \alpha\beta\delta + \beta\gamma\delta = R,$$

$$\alpha\beta\gamma\delta = S.$$

Then for the bitangents $\alpha = \beta$, $\gamma = \delta$,

$$2(\alpha + \gamma) = P,$$

$$\alpha^2 + 4\alpha\gamma + \gamma^2 = Q,$$

$$2\alpha\gamma(\alpha + \gamma) = R,$$

$$\alpha\beta\gamma\delta = \alpha^2\gamma^2 = S,$$

$$(\alpha + \gamma)^2 + 2\alpha\gamma = Q, \text{ or } \alpha\gamma = \frac{Q}{2} - \frac{P^2}{8},$$

$$P\left(\frac{Q}{2} - \frac{P^2}{8}\right) = R, \quad \alpha\gamma = \frac{R}{P},$$

*and therefore* $$\frac{R^2}{P^2} = S.$$

By means of the two equations—

$$P\left(\frac{Q}{2}-\frac{P^2}{8}\right) = R, \qquad \frac{R^2}{P^2} = S,$$

since P, Q, R, S are functions of $m$ and $a$, we determine the double tangents.

We may also use the above equations to determine the two tangentials of any single tangent of a quartic in the point where the tangent meets the curve again. In this case we assume $m, a, \alpha$ as known, and we have—

$$2\alpha + \gamma + \delta = P,$$

$$\alpha^2 + 2\alpha\gamma + 2\alpha\delta + \gamma\delta = Q,$$

rom which the co-ordinates of the tangentials may be determined by he solution of a quadratic equation.

We next proceed to find the equations which determine the bitanents of the quintic. We substitute $y = mx + a$ in the general quation, and obtain (using the same notation)—

$$\alpha + \beta + \gamma + \delta + \mu = P,$$

$$\alpha\beta + \alpha\gamma + \alpha\delta + \alpha\mu + \beta\gamma + \beta\delta + \beta\mu + \gamma\delta + \gamma\mu + \delta\mu = Q,$$

$$\beta\gamma + \alpha\beta\delta + \alpha\beta\mu + \alpha\gamma\delta + \alpha\gamma\mu + \alpha\delta\mu + \beta\gamma\delta + \beta\gamma\mu + \beta\delta\mu + \gamma\delta\mu = R,$$

$$\alpha\beta\gamma\delta + \alpha\beta\gamma\mu + \alpha\beta\delta\mu + \alpha\gamma\delta\mu + \beta\gamma\delta\mu = S,$$

$$\alpha\beta\gamma\delta\mu = T.$$

Put $\alpha = \beta$, $\gamma = \delta$, then the equations become—

$$2(\alpha + \gamma) + \mu = P,$$

$$\alpha^2 + \gamma^2 + 2(\alpha + \gamma)\,\mu + 4\alpha\gamma = Q,$$

$$2\alpha^2\gamma + 2\alpha\gamma^2 + (\alpha^2 + \gamma^2)\,\mu + 4\alpha\gamma\mu = R,$$

$$\alpha^2\gamma^2 + 2\alpha\gamma(\alpha + \gamma)\,\mu = S, \qquad \alpha^2\gamma^2\mu = T.$$

Hence $$\alpha + \gamma = \frac{P - \mu}{2},$$

$$(\alpha + \gamma + \mu)^2 + 2\alpha\gamma = Q + \mu^2,$$

$$2\alpha\gamma = Q - \frac{P^2}{4} - \frac{P\mu}{2} + \frac{3\mu^2}{4}.$$

*Hence the remaining* equations become—

$$\left(Q - \frac{P^2}{4} - \frac{P\mu}{2} + \frac{3\mu^2}{2}\right)\left(\frac{P+\mu}{2}\right) + \mu\left(\frac{P-\mu}{2}\right)^2 = R,$$

$$\frac{1}{4}\left(Q - \frac{P^2}{4} - \frac{P\mu}{2} + \frac{3\mu^2}{4}\right)^2 + \left(Q - \frac{P^2}{4} - \frac{P\mu}{2} + \frac{3\mu^2}{4}\right)\left(\frac{P-\mu}{2}\right) = S,$$

$$\frac{\mu}{4}\left(Q - \frac{P^2}{4} - \frac{P\mu}{2} + \frac{3\mu^2}{4}\right)^2 = T.$$

We have to eliminate $\mu$ between these three equations; the resultant between equations of the third and fourth order is given by Salmon; also the resultant between two quartics, from which we may deduce the resultant of a quartic and a quintic. The result will be tremendously complicated; but we must remember the number of double tangents to a non-singular quintic is 120, which naturally suggests an equation of the 120th degree, which I apprehend few mathematicians would like to solve. It is impossible, however, to predict the future of analysis.

I have omitted to take any notice in this paper of the modifications which would be occasioned by double points, hoping, if permitted, to return to the subject.

I would observe in conclusion that the same method applies to the determination of points of inflexion. Thus in the quartic, taking $\alpha, \beta, \gamma, \delta$ for the roots of the equation produced by eliminating between the quartic and a straight line, and putting $\alpha = \beta = \gamma$, we find it easy to eliminate $\alpha$ and $\delta$ and to find two equations which will give the inflexional tangents.

## XIV. "On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of August 28–29, 1886. Preliminary Notice." By Captain W. DE W. ABNEY, C.B., R.E., F.R.S., and T. E. THORPE, Ph.D., F.R.S. Received June 21, 1888.

Attempts to measure the brightness of the corona were made by Pickering in 1870, and by Langley and Smith, independently, in 1878, with the result of showing that the amount of emitted light as observed at various eclipses, may vary within comparatively wide limits. These observations have been discussed by Harkness ('Washington Observations for 1876,' Appendix III), and they will be again discussed in the present paper. Combining the observations it appears that the total light of the corona in 1878 was 0·072 of that of a standard candle at 1 foot distance, or 3·8 times that of the full moon, or 0·0000069 that of the sun. It further appears from the photographs that the coronal light varied inversely as the square of

the distance from the sun's limb. Probably the brightest part of the corona was about 15 times brighter than the surface of the full moon, or 37,000 times fainter than the surface of the sun.

The instruments employed by the authors in the measurement of the coronal light on the occasion of the solar eclipse of August 28–29, 1886, were three in number. The first was constructed to measure the comparative brightness of the corona at different distances from the moon's limb. The second was designed to measure the total brightness of the corona, excluding as far as possible the sky effect. The third was intended to measure the brightness of the sky in the direction of the eclipsed sun. In all three methods the principle of the Bunsen photometric method was adopted, and in each the comparison-light was a small glow-lamp previously standardised by a method already described by one of the authors in conjunction with General Festing. In the first two methods the photometer-screen was fixed, the intensity of the comparison-light being adjusted by one of Varley's carbon resistances: in the third the glow-lamp was maintained at a constant brightness, the position of the screen being adjusted along a graduated photometer bar, as in the ordinary Bunsen method. Full details of the construction of the several pieces of apparatus will be given in the full paper.

The observations during the eclipse were made at Hog Island—a small islet at the south end of Grenada, in lat. 12° 0′ N. and long. 61° 43′ 45″ W., with the assistance of Captain Archer and Lieutenants Douglas and Bairnsfather of H.M.S. "Fantôme." The duration of totality at the place of observation was about 230 seconds, but measurements were possible only during 160 seconds, at the expiration of which time the corona was clouded over. A careful discussion of the three sets of measurements renders it almost certain that the corona was partially obscured by haze during the last 100 seconds that it was actually visible. Selecting the observations made during the first minute, which are perfectly concordant, the authors obtain six measurements of the photometric intensity of the coronal light at varying distances from the sun's limb, from which they are able to deduce a first approximation to the law which connects the intensity of the light with the distance from the limb.

The observations with the integrating apparatus made independently by Lieutenants Douglas and Bairnsfather, agree very closely. It appears from their measurements that the total light of the corona in the 1886 eclipse was—

| | | |
|---|---|---|
| Douglas .............. | 0·0123 | standard candle. |
| Bairnsfather .......... | 0·0125 | " |
| *Mean* ........ | 0·0124 | |

*at a distance of 1 foot.*

In comparing these observations with those made during the 1878 eclipse, it must be remembered that the conditions of observation on the two occasions were widely different. The observations in the West Indies were made at the sea's level, in a perfectly humid atmosphere and with the sun at no greater altitude than 19°. Professor Langley, in 1878, observed from the summit of Pike's Peak in the Rocky Mountains at an altitude of 14,000 feet, in a relatively dry atmosphere and with the sun at an altitude of 39°.

From observations on the transmission of sunlight through the earth's atmosphere (Abney, 'Phil. Trans.,' A, vol. 178 (1887), p. 251) one of the authors has developed the law of the extinction of light, and, by applying the necessary factors, it is found that the intensity of the light during the 1886 eclipse, as observed at Grenada, is almost exactly half of that of which would have been transmitted from a corona of the same intrinsic brightness when observed at Pike's Peak. Hence to make the observations of Professor Langley comparable with those of the authors, the numbers denoting the photometric intensity of the corona in 1878 must be halved. The result appears, therefore, that whereas in 1878 the brightness of the corona was 0·0305 of a standard candle at a distance of 1 foot, in 1886 it was only 0·0124 of a candle at the same distance. Several of the observers of the West Indian Eclipse (including one of the authors) were also present at the eclipse of 1878, and they concur in the opinion that the darkness during the 1886 eclipse was very much greater than in that of 1878. The graduations on instruments, chronometer faces, &c., which were easily read in 1878, were barely visible in 1886. In explanation of this difference in luminous intensity it must not be forgotten that the 1878 eclipse was not very far removed from a period of maximum disturbance, whereas in 1886 we were approaching a period of minimum disturbance.

## XV. "Seismometric Measurements of the Vibration of the New Tay Bridge during the Passing of Railway Trains." By J. A. Ewing, B.Sc., F.R.S., Professor of Engineering in University College, Dundee. Received June 20, 1888.

The absolute methods of seismometry which have been developed during recent years in Japan, and have been applied to the measurement of earthquakes there and elsewhere, may serve a useful purpose in determining the extent and manner of the shaking to which engineering structures are subject through storms of wind, moving loads, or other causes of disturbance. Existing forms of seismograph are well suited for measurements of this kind, provided the frequency

f the vibrations to be measured is neither very much greater nor ery much less than is usual in earthquakes, and provided, of course, he amplitude of vibration does not exceed the capacity of the instrument. For vibrations of high frequency a greater rigidity in the multiplying and recording apparatus would be necessary; in vibrations of very long period, on the other hand, the mass whose inertia furnishes the steady-point of reference will not remain at rest. Between these extremes, however, there is a wide range within which such seismographs as are now used to measure earthquakes may be trusted to give a record that is correct in all substantial particulars, and the vibrations to be referred to below fall within this range.

The writer has recently employed his Duplex Pendulum Seismograph to examine the vibration of the new Tay Bridge while railway trains are passing over it, facilities for this examination having been kindly given by Mr. Fletcher F. S. Kelsey, resident representative of Messrs. Barlow, the engineers of the bridge. The results are perhaps worth publishing, not so much for any interest they have in hemselves, as because they exemplify a novel method of inquiry which may prove of use in other cases to engineers. The duplex pendulum seismograph, which was designed for and applied to the neasurement of earthquakes in Japan in 1882,* consists essentially of pair of masses which are supported and connected in such a manner hat they form an astatic combination with freedom to move in any orizontal direction. One of the two is hung from above and is table; the other is supported from below and is unstable; and the wo are constrained to move together by a ball-and-tube coupling. Their equilibrium is adjusted to be very nearly neutral, and this fits hem to furnish a steady-point with respect to which motion of the ground in any azimuth may be recorded and measured. The motion s recorded by a lever, the marking point of which draws a magnified opy of the horizontal motion of the ground upon a smoked-glass plate. Fig. 1 shows the construction of the duplex pendulum seismograph as used in these experiments, and as now made by the Cambridge Scientific Instrument Company for earthquake observatories. The stable mass is a disk of lead *a* cased in brass (shown in ection in fig. 1) hung by three parallel wires from the top of the containng box. This trifilar suspension has several advantages over the usual uspension of a pendulum from a single point; in particular it prevents wisting about a vertical axis. The unstable or inverted pendulum *b* is lso a disk of lead below the other, and is held up by a tubular strut which ends in a hard steel point resting in an agate socket in the

* See 'Transactions of the Seismological Society of Japan,' vol. 5, p. 89, or the uthor's *memoir on* "Earthquake Measurement" ('Memoirs of the Science epartment of the University of Tokio,' No. 9, 1883).

[redacted]



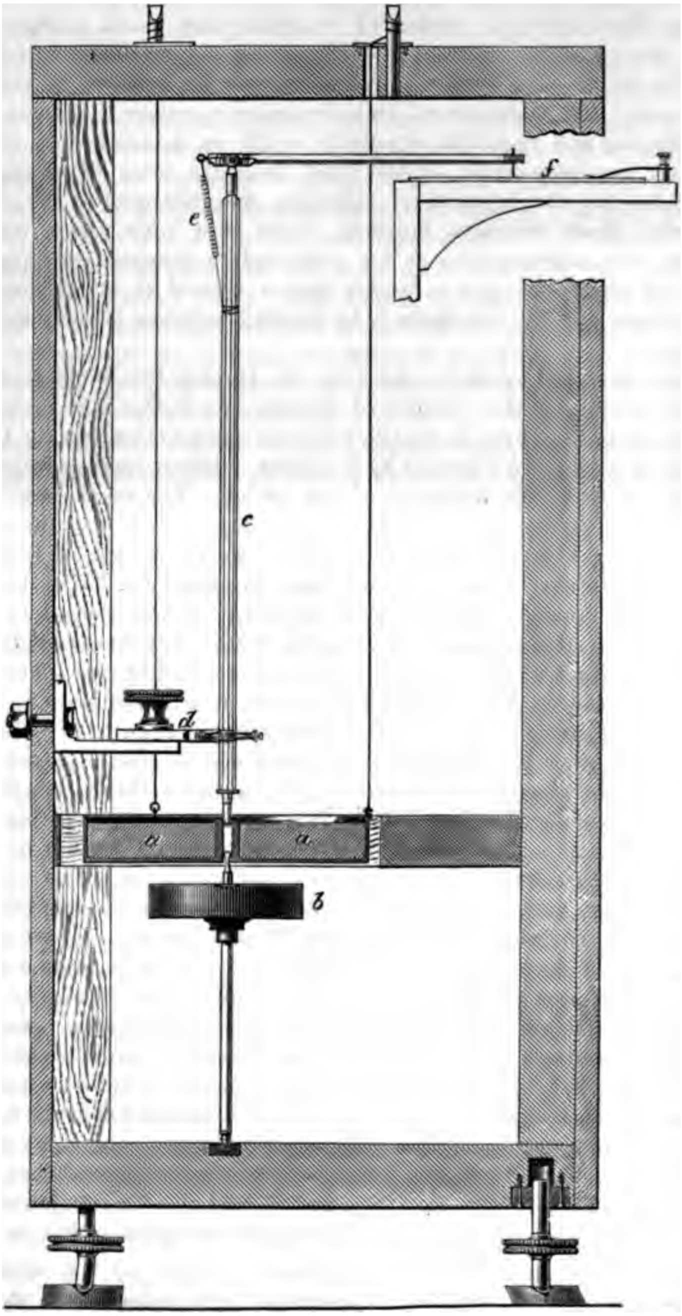



*Fig. 1.*—Section through Duplex Pendulum Seismograph. (Scale ¼.)

base of the box. A small brass ball attached to the lower mass $b$ fits easily but without shake in a cylindrical hole in $a$, so that the two must swing together. The masses of $a$ and $b$ are proportioned, with respect to their distances from their respective supports, so that the equilibrium of the compound system is nearly neutral, and by way of final adjustment the upper disk $a$ may be raised or lowered by turning the pins at the top until the margin of stability is as small as may be wished. The recording lever $c$ is held by a gimbal joint in a bracket $d$, fixed to the side of the box, and capable of adjustment vertically and horizontally. The bottom of the lever is a ball which gears into the hole in $a$, and at the top there is a hinged index of straw with a needle-point to write the record. To reduce friction, part of the weight of the straw is borne by a spring $e$. The smoked glass plate $f$ stands on a shelf which projects from one of the sides of the case, which is a triangular box. In the particular instrument employed at the Tay Bridge, the ground's motion was magnified six times.

The seismograph was set upon the ground in the six-foot way between the two pairs of rails at the middle of the length of the southernmost high girder, at a distance of about 1¼ mile from the Dundee end of the bridge, and ¾ mile from the Fife end. The girders are there 245 feet long, and stand at a height of about 110 feet above the bottom of the river and 135 feet above the foundations of the piers. Between this and the Fife shore there are 28 piers; towards Dundee there are 57 piers, and at that end the bridge forms a curve of 21 chains radius by which its direction is turned through nearly a right angle as it approaches the shore.* In this position observations were made while eight trains crossed the bridge. There was no wind, and, until a train came on, the recording index of the seismograph stood perfectly at rest.

As soon, however, as a train entered the bridge—from either end—the index began to move. The movements were at first so minute that it was difficult to estimate their range with any accuracy; allowing for the multiplication given by the lever, the movement began with longitudinal shaking through something like $\frac{1}{800}$ of an inch. In the case of trains coming from Dundee this was transmitted round the bend of the bridge and was noticed long before the train had reached the straight part. At first the movement was wholly longitudinal, and it was not until the train had come much nearer that lateral oscillation began to be felt. The interval by which longitudinal vibrations preceded transverse vibrations was much greater than could be explained by difference in their velocity of transmission. Near the

* For particulars of the dimensions of the bridge, reference should be made to *Mr. Kelsey's paper in the* 'Proceedings of the Institute of Mechanical Engineers,' *August, 1887.*

source of disturbance (as one learnt later when the train was passing the seismograph) the lateral movement was actually greater than the longitudinal; it appeared, therefore, that longitudinal disturbance reached the instrument from greater distances than lateral disturbance, because it was transmitted along the bridge with less loss. As the train came nearer, lateral movements became superposed on the longitudinal ones, and the index of the seismograph described an immense series of irregular loops, the range of which increased at first slowly and then quickly to a maximum as the train passed the instrument. Along with this progressive increase there was a periodic rise and fall in amplitude, the beat of which apparently agreed with the interval taken by the train to pass from pier to pier over successive spans. The last faint movements terminated abruptly when the train cleared the structure.

The vibrations were too numerous to allow the diagrams drawn by the seismograph to be at all clear, and a better idea of the motion was to be got by watching the index than by subsequent examination of the record. Fig. 2 reproduces two of the diagrams, and is sufficiently

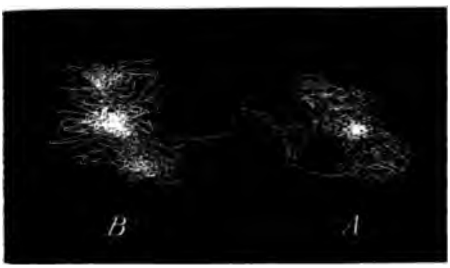



FIG. 2.—Tay Bridge vibrations, recorded by Duplex Pendulum Seismograph.

representative of the rest. As the figure is printed, the top and bottom are in the longitudinal direction of the bridge. Of the two, the figure marked A was drawn first by a passenger train coming from the south end: after it had passed the seismograph and when the oscillations were again small, it was observed that another train had entered the bridge from Dundee. The glass plate was accordingly moved by hand to a new position, and the second diagram (B) was obtained. The movements were in general of the form of nearly closed loops resembling ellipses—showing that the periods of lateral and of longitudinal vibration did not differ greatly. In the greatest movements the loops are much wider in the lateral than in the longitudinal direction. The greatest lateral movement appears to have been about one-tenth, certainly not more than one-eighth of an inch; the greatest longitudinal movement about one-fourth of this. There were about three complete vibrations per second.

The seismograph was afterwards set up just above the pier at the uth end of the span in the middle of which it had previously been anding, and five more records were obtained in the new position. xcept that the motion was somewhat less, they had much the same iaracteristics as before. The following notes refer to the passage of slow goods-train from Dundee as observed from this position :—

| Mins. | Secs. | |
|---|---|---|
| 30 | 40 | Train entered bridge: minute longitudinal oscillation began. |
| 32 | 0 | Train entered straight portion of bridge. |
| 33 | 0 | Lateral oscillation began. |
| 36 | 0 | Train passed seismograph. |
| 38 | 10 | Tail van of train off bridge: oscillation ceased. |

In all seismometric work, whether it be the measurement of earth- |uakes proper, or of such shakings as these, the trustworthiness of he record depends on the degree to which the presumed "steady- oint" of the instrument remains at rest during a protracted dis- urbance of the base. The accuracy of a seismograph admits of asy experimental test in the manner which the author described nd illustrated when communicating to the Royal Society an account f his Horizontal Pendulum Seismograph, for recording separate omponents of motion upon a moving plate.* The test consists in lacing the instrument upon a stand which may be shaken by hand, nd causing a true autograph of the motion of the stand to be drawn y an independently supported index, side by side with the record iat is drawn by the seismograph itself. Fig. 3 shows how this test as applied to the instrument with which the Tay Bridge observa- ons were made. The seismograph was mounted on a stand which as constructed to give it two degrees of freedom of horizontal anslation, without freedom to rotate. This was done by laying a air of turned steel rollers parallel to each other on the top of a steady vel table; a small drawing-board rested on them; on the top of it second pair of steel rollers were laid at right angles to the pair elow; a second small drawing-board lay on them, and the istrument stood upon it. The upper board was then free for anslation in all azimuths, and was shaken by hand so that it nitated the motion in an actual earthquake. A record of this otion was drawn by the seismograph index, and beside it a second cord was drawn by the lever and index *g* (fig. 3) which was held y a gimbal joint in a stiff bracket *h* secured to the upper board, and ok its motion from a true steady-point *i* obtained by making the ottom end of the lever in the form of a small ball socketed in a

* "*On a new Seismograph,*" 'Roy. Soc. Proc.,' vol. 31, 1881, p. 440.

cylindrical hole in the bracket $j$, which was firmly fixed to the (motionless) top of the table. When the multiplication given by this lever $g$ is arranged to be the same as that given by the seismograph the two records should be identical, except for error caused by the "steady-point" of the seismograph wandering through friction, or because of the stability of the suspended mass, and except for those errors which both the seismograph and the testing lever are liable to

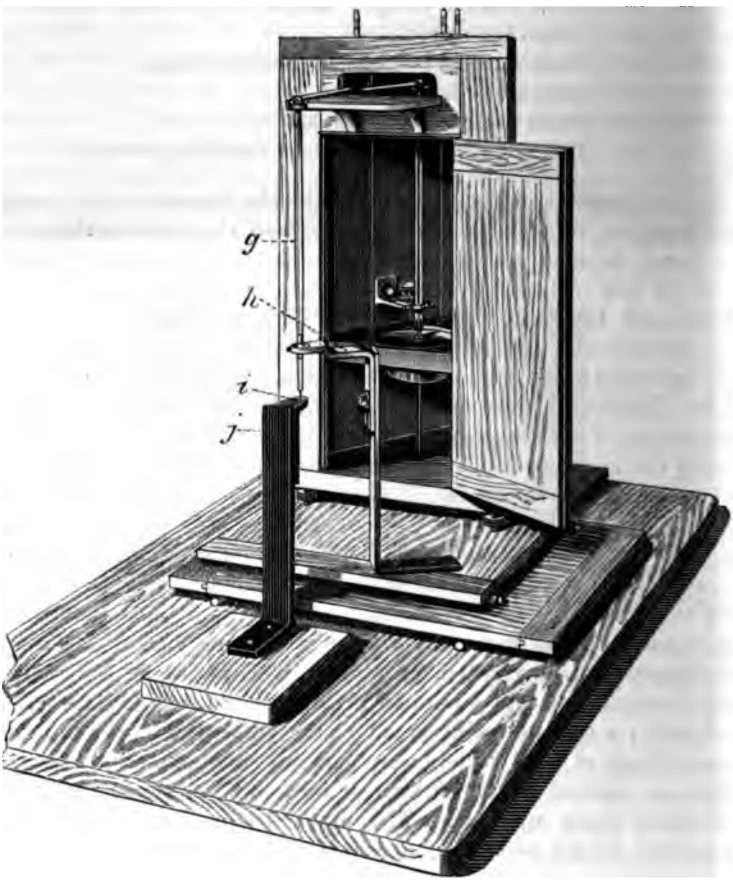



FIG. 3.—Arrangement for testing the Duplex Pendulum Seismograph.

through backlash at the joints and want of rigidity in the lever and index arm. In practice the agreement between the records is most satisfactory. Fig. 4 gives examples of the result of this test as applied to the seismograph which was used upon the Tay Bridge, when the shaking was made to imitate such movements as the

ground executes in small and in large earthquakes. Tests of this kind not only demonstrate the accuracy of the seismograph, but are a convenient means of finding experimentally the ratio in which the recording index multiplies the motion of the ground.

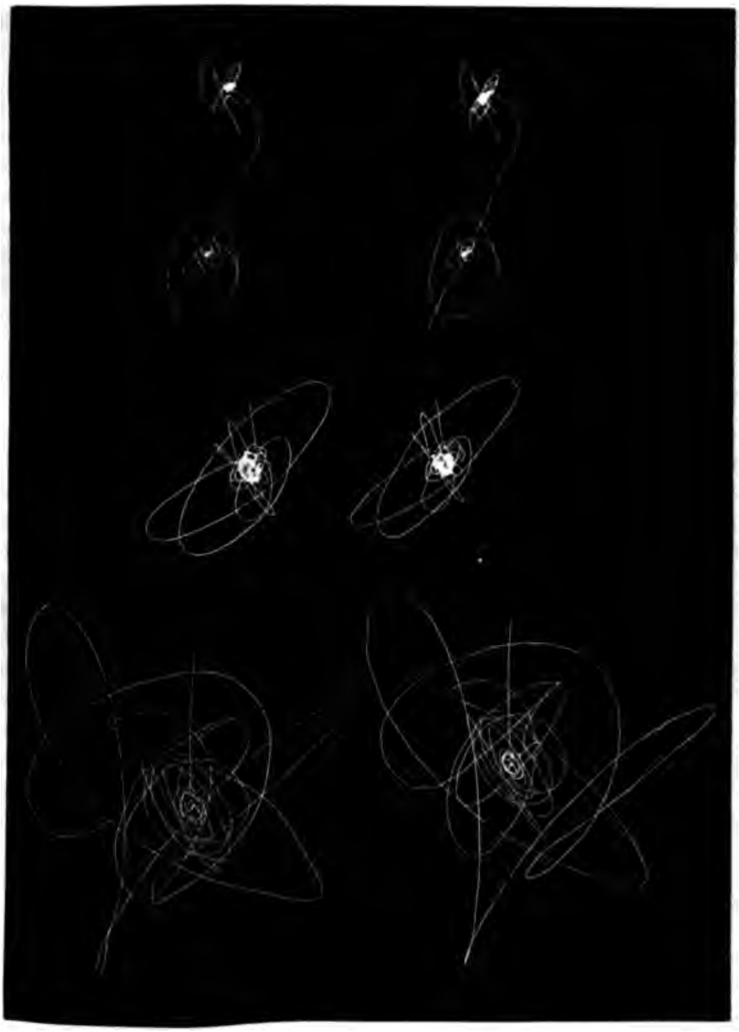


Fig. 4.—Comparison diagrams to test accuracy of Duplex Pendulum Seismograph.

For an exhaustive examination of the vibration of a structure under "live" loads, the more elaborate type of seismograph might be used, which records linear components of the motion on a surface

that is moved uniformly by clockwork. The usual form of this instrument comprises two horizontal pendulums, for the two horizontal components, and a third piece which is suspended astatically with freedom to move up and down only for the vertical component.* This arrangement employs a distinct mass and a distinct "steady-point" with respect to each component. The duplex pendulum may, however, be modified, or rather supplemented, so that it records two components of horizontal motion separately (on a moving surface) by attaching to one or other of the bobs a pair of slot guides at right angles to the direction of the two components, and pivotting in these the short ends of a pair of recording levers, so that each lever will be moved when the bob moves across the direction of the corresponding slot, but will not be moved when the bob moves along that direction. This makes a compact form of two-component horizontal seismograph, with the advantage that by retaining the ordinary index we have, in addition to the components, a plan drawn of the whole shaking. For the vertical component it is convenient to have a distinct astatically hung mass. But, as a sort of *tour de force* in astatic suspension, one or other of the bobs of the duplex pendulum may be allowed to have a limited amount of vertical freedom, and may have its equilibrium made nearly neutral for vertical displacements as well as for horizontal displacements. Let the upper bob, for instance, be hung from a platform which is free to rise and fall by rotating about a horizontal axis, and which is held up by springs. By applying the pull of the springs in such a manner that the moment of the pull about that axis is always nearly equal to the moment of the weight, we may approach vertical astaticism as closely as may be wished, and, provided the movements up and down are not too great to interfere with the proper gearing of the bobs, the mass will then possess universal freedom of translation, with nearly neutral equilibrium for all directions of displacement. In practical seismometry, however, it is no doubt advisable to restrict the freedom of the suspended mass to (at most) two degrees.

The Society adjourned over the Long Vacation to Thursday, November 15th.

* *See* 'Transactions of the Seismological Society of Japan,' vol. 3 (1881), p. 140, *or the author's* memoir on "Earthquake Measurement" cited above. A complete *three-component* instrument is described in 'Nature,' vol. 34, p. 343.

*Presents, June* 21, 1888.

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"On the Relations of the Diurnal Barometric Maxima to certain critical Conditions of Temperature, Cloud, and Rainfall." By HENRY F. BLANFORD, F.R.S. Received March 30,—Read May 3, 1888.

It is not my purpose in this paper to discuss the general problem of the diurnal barometric variation. It is certainly a very complex phenomenon, and one of which no satisfactory analysis has yet been made. The atmospheric stress (whatever be its nature) that originates the oscillation, is followed by movements which alter both the vertical and horizontal distribution of the gravitating mass, and the striking differences that characterise the diurnal curve of pressure on mountain peaks, plains, and valleys, and on the ocean as compared with the land, are doubtless due in a large measure to these resulting redistributions of the mass.

Amid all the recorded variations of the oscillation as a whole, the feature that displays the greatest constancy is the occurrence of a maximum in some hour of the forenoon, and of a second maximum one or two hours before midnight. The exceptional cases, in which these two critical phases are much shifted from their normal positions, are but few, and may probably all be explained by gravitation effects being superadded to the normal semidiurnal oscillation.

One of the most anomalous forms of the diurnal oscillation yet recorded is that given by Professor Mohn, for the North Atlantic, between latitudes 62° and 80°, in the summer months.* The general form of this pressure curve is similar to that of the diurnal temperature curve. It falls to a minimum in the early morning hours, and rises to a maximum between 1 h. and 3 h. 30 m. P.M. But of the three curves for different years and latitudes given by Professor Mohn, two show, as a subordinate feature, a small rise to a secondary maximum, between 10 and 11 P.M., and two an irregularity in the morning rise, such as would result from a small wave with a maximum about 7 or 8 A.M., in combination with the principal oscillation of twenty-four hours' period. At Christiania and Upsala the phases of the single period oscillation are reversed, the maximum being in the night, the minimum in the day, but the semidiurnal element exhibits characters similar to those of the North Atlantic curve.

This comparative constancy of the semidiurnal element of the oscillation, which was originally pointed out by Lamont,† seems to indicate that it depends more directly on the action of the sun than

* 'Norske Nordhavs Expedition,' 1876–1878.

† 'Sitzungsber. d. Bayerisch. Akademie,' 1862, vol. 1, p. 89.

the diurnal element, and that its explanation is a first necessary to that of the whole phenomenon. The object of the present r is to draw attention to the approximate coincidence of its imum phases with certain critical phases of temperature, cloud, rainfall, which may at least help to throw some light on its ical causes.

*Forenoon Maximum.*

was noticed independently by Espy, in 1840,* Davies, in 1859,† Kreil, in 1861,‡ that the forenoon maximum of the barometric lation approximately coincides with the most rapid rise of temture, and each of these writers attributed the rise of pressure to eactionary effect of the heated and expanding atmosphere. The data, however, given by any of them in support of the state- t are the horary variations of the temperature and pressure at ue, by Kreil, and a rough diagram of the diurnal curves at Padua, avies; and shortly after the publication of Kreil's paper, the subject very fully discussed by Lamont, in the paper already referred the 'Sitzungsberichte' of the Bavarian Academy, wherein he ed that, on the ordinary assumption that the atmosphere is free pand in a vertical direction, against no other resistance than the pressure of the superincumbent mass, the supposed reactionary would be inappreciable.

nce the publication of Lamont's paper, I am not aware that any icist has paid further attention to the hypothesis in question, or ght it worth while to appeal to further evidence in verification e observation on which it is based, until quite recently. But in , in noticing the subject of the barometric oscillation in the ian Meteorologist's Vade Mecum,' it occurred to me that Lamont's nption that the atmosphere is free to expand vertically, lifting superincumbent mass, is subject to an important modification h may greatly alter the conditions of the problem as contemplated m.

ese conditions take no account of the resistance to expansion must be opposed by the highly attenuated but extremely cold nal atmospheric strata of great but unknown thickness, the ence of which is proved by the phenomena of luminous meteors. a sheet of the atmospheric envelope, of indefinite horizontal t, resting on the earth's surface, be heated and charged with r, the first effect will be an increase of its elastic tension, which e relieved by a wave of elastic compression transmitted to the

* 'Brit. Assoc. Rep.,' 1840, Part II, p. 55.

† '*Edinburgh Phil. Journ.*,' vol. 10, 1859, p. 225.

‡ '*Wien. Akad. Sitzungsber.*,' vol. 43 (Abth. 2), p. 121.

2 H 2

overlying strata. Having regard to the slow rate at which this wave is generated, the rise of temperature, even in such a climate as that of Northern India, not exceeding 5° or 6° in the hour of most rapid heating, equivalent to an increment of less than $\frac{1}{100}$th of the initial pressure, it appears to me that the rate of propagation will be sensibly that due to half the height of a homogeneous atmosphere, or a little more than two-thirds the rate of the sound-wave. This rate will be continually retarded as the wave advances through the loftier and colder strata, being proportional to the square root of the absolute temperature of each stratum. And it will depend on the thickness of the atmospheric sheet heated, the amount of the heating, and on the thickness and temperature of the cold external strata, whether the retardation may not be such as to allow of the tension of the lower strata becoming such as is indicated by the barometer at the time of the forenoon maximum. Under such circumstances, the instant of maximum pressure should coincide with that of the most rapid rise of temperature and vaporisation.

I do not think that our knowledge of any of these fundamental conditions is such as to justify a rejection of the hypothesis on *à priori* grounds, and it may therefore be worthy of inquiry how far it is in accordance with verifiable observation. At Calcutta, the atmospheric pressure at 9 h. 30 m. A.M. is about $\frac{1}{300}$th greater than at the time of the morning minimum; an increase which would be produced by heating the air in a closed vessel less than 2°. A retardation of about half an hour in the dissipation of the increased pressure produced by heating and evaporation would suffice to produce the observed effect.

Dr. Sprung, in his admirable manual, the 'Lehrbuch der Meteorologie,' published in 1885, has referred to the above hypothesis,* and has tested the coincidence of the critical phases of temperature and pressure by the summer results of the hourly observations and autographic registers of the Prague Observatory, from 1842 to 1861, which have been recomputed by Professor Augustin. The result of this test appears to be satisfactory. At Prague, on the mean of the summer months, the forenoon barometric maximum occurs a little after 8 A.M., and nearly coincides with the most rapid rise of temperature.†

In India there is no station at which the forenoon maximum falls at so early an hour at any season; but at Yarkand and Kashghar, according to Dr. Scully's valuable observations, in the summer, it occurs even earlier than at Prague, while in the winter it is as late as the mean epoch at Calcutta. It is true we have only fifteen series of

* *Op. cit.*, p. 336.

† *As* computed from the figures given by Dr. Sprung, by the application of the *method* of differences (see footnote below), the barometric maximum occurs *nineteen* minutes later than the instant of most rapid heating.

hourly readings for the winter months, November to February, taken at intervals of seven or eight days, and but eight series for June and July, but so regular is the march of the diurnal variation both of temperature and pressure in this climate, that even these suffice to show the distinctive characters of the curves at both seasons. The observations have been published at length in the first volume of the 'Indian Meteorological Memoirs.'* To eliminate small irregularities, corrected hourly values have been computed from these by means of the harmonic formula. A very exact determination of the critical phases cannot of course be expected from such data, but according to the method of computation adopted,† the epochs of the forenoon pressure maximum and of most rapid heating are as follow at the two seasons :—

| | Max. rise temp. | Bar. max. |
|---|---|---|
| Winter months.... | 9 h. 38 m. A.M. | 9 h. 36 m. A.M. |
| Summer months .. | 7 h. 56 m. „ | 7 h. 38 m. „ |

* *Op. et vol. cit.*, p. 94, *et seq.*

† Throughout this paper the time of most rapid heating has been determined in the following manner: in general, from the uncorrected means of the observations, which, for the reasons shown by Dr. Bergsma ('Batavia Mag. and Met. Obs.,' vol. 1, p. xvii) and in accordance with my own experience and that of other Indian meteorologists, if the observations are sufficiently extensive, are more trustworthy than the so-called corrected values obtained by computing them from three or four terms of the harmonic formula.

The instant of most rapid rise of temperature may be ascertained by twice differentiating for the values of $t$ the formula which expresses the temperature $\theta$ as a function of the time $t$, and putting

$$\frac{d^2\theta}{dt^2} = 0.$$

The most convenient formula for this purpose is that of the method of differences employed by Dr. Jelinek for obtaining the approximate times of the maximum and minimum phases of temperature, pressure, &c. On taking the first, second, and third differences of the temperatures at the clock hours, two before and two after the instant of most rapid rise, the hour in which this occurs is shown by the change of algebraical sign of the second order of differences. Denoting that which precedes this change by $\Delta_2$, and the differences of the first and third order next following in order of sequence by $\Delta'$ and $\Delta'_3$, the second differentiation of the formula

$$\theta = a + t\Delta'_1 + \frac{t\,(t-1)}{2}\,\Delta_2 + \frac{(t+1)\,t\,(t-1)}{6}\,\Delta'_3 + \ldots$$

neglecting the higher terms, gives

$$\frac{d^2\theta}{dt^2} = \Delta_2 + t\Delta'_3 = 0,$$

whence

$$t = -\frac{\Delta_2}{\Delta'_3},$$

which value of $t$ reckons from the clock hour corresponding to $\Delta_2$. The epoch thus obtained has an error of a few minutes only, and is quite accurate enough for the present purpose.

Considering the character of the data and the method of comp this close coincidence in the winter months must be regarde some degree fortuitous.

In order to test the hypothesis more thoroughly, I have four stations, the data for which are more ample, and tho trustworthy, viz., Bombay, Calcutta, Batavia, and Melbourne; the case of the last three, I have compared the critical ph question for every month of the year.

The data for Bombay are taken from Mr. C. Chambers's vol the Meteorology of the Bombay Presidency. The barometric Calcutta are extracted from vol. 1 of the 'Indian Meteor Memoirs,' and as I have not the corresponding thermometri at hand,* I have substituted those obtained from the measuren the Alipore photographic traces for the six years 1881–1885. latter relate, therefore, to a different and later series of years, furnished by a different observatory, but this is hardly a m importance. The Batavian data are taken from the first vol Dr. Bergsma's 'Magnetical and Meteorological Observation have been derived from the hourly readings of three years; Melbourne data are from Dr. Neumayer's discussion of the o tions of the Flagstaff Observatory. They extend over five The results are shown in the following table:—

Bombay.

| | Max. rise temp. | Bar. max. | In |
|---|---|---|---|
| April to September, mean | 7 h. 45 m. A.M. | 9 h. 43 m. A.M. | 1 h |
| October to March „ | 7 h. 53 m. „ | 9 h. 31 m. „ | 1 h |

* *Since* the reading of the paper before the Society I have receiv *Calcutta* the mean values of the horary readings of the thermometer, corre *to those of* the barometer here dealt with. See appended note at the e *paper.*

| | Calcutta. | | | Batavia. | | | Melbourne. | | |
|---|---|---|---|---|---|---|---|---|---|
| | Max. rise temp. | Bar. max. | Interval. | Max. rise temp. | Bar. max. | Interval. | Max. rise temp. | Bar. max. | Interval. |
| January | 8 h. 41 m. | 9 h. 44 m. | 1 h. 3 m. | 8 h. 36 m. | 9 h. 22 m. | 0 h. 46 m. | 7 h. 7 m. | 8 h. 33 m. | 1 h. 26 m. |
| February | 8 36 | 9 52 | 1 16 | 8 34 | 9 26 | 0 52 | 7 50 | 9 5 | 1 15 |
| March | 8 16 | 9 47 | 1 31 | 8 33 | 9 15 | 0 42 | 8 24 | 9 2 | 0 38 |
| April | 8 10 | 9 35 | 1 25 | 8 36 | 9 13 | 0 37 | 8 43 | 9 25 | 0 42 |
| May | 7 11 | 9 23 | 2 12 | 8 38 | 9 8 | 0 30 | 9 5 | 9 18 | 0 13 |
| June | 6 52 | 9 22 | 2 30 | 8 34 | 9 4 | 0 30 | 9 59 | 9 29 | −0 30* |
| July | 8 15 | 9 33 | 1 18 | 8 46 | 9 11 | 0 25 | 9 31 | 9 30 | −0 1 |
| August | 8 28 | 9 38 | 1 10 | 8 44 | 9 2 | 0 18 | 8 51 | 9 20 | 0 29 |
| September | 7 40 | 9 33 | 1 53 | 8 22 | 8 54 | 0 32 | 7 58 | 8 52 | 0 54 |
| October | 8 2 | 9 25 | 1 23 | 7 56 | 8 59 | 1 3 | 6 56 | 8 51 | 1 55 |
| November | 8 23 | 9 24 | 1 1 | 8 1 | 8 58 | 0 57 | 6 42 | 8 27 | 1 45 |
| December | 8 36 | 9 34 | 0 58 | 8 33 | 9 14 | 0 41 | 6 49 | 8 34 | 1 55 |
| Year | 8 27 | 9 35 | 1 8 | 8 33 | 9 8 | 0 35 | 8 36 | 9 7 | 0 31 |

* The minus sign indicates that the maximum of pressure precedes that of the temperature rise.

From this table it appears that only in one or two of the winter months at Batavia and Melbourne does the forenoon maximum of pressure coincide so nearly with the moment of most rapid heating as at Prague and Yarkand. In all cases, except in the midwinter months at Melbourne, the former follows the latter by an interval which averages 31 minutes at Melbourne, 35 minutes at Batavia, 1 hour and 8 minutes at Calcutta, and 1 hour and 48 minutes at Bombay. But it is to be noticed that, at all the stations, this interval is shortest in the winter and greatest in the summer. It is true that the computed temperature epochs may be 10 minutes or so in error, owing to the merely approximative character of the method adopted, and that, for an hour or more afterwards, the change in the rate of rise is very small, not exceeding a few tenths of a degree per hour; but the retardation of the barometric maximum is too systematic to be explained away by any such considerations. There are, however, others of a very obvious character.

The hypothesis attributes the increase of pressure in the forenoon to the mean increase of tension in the atmosphere up to a very great height, not to that of the lowest stratum only. And since this latter is heated much more rapidly than the higher strata, and that, owing to variations in the character of the earth's surface, the rates of heating in contiguous areas of the lower strata themselves vary indefinitely, the convective movements, which are set up in consequence, produce innumerable small modifications in the form of the local temperature curves, which will to a great extent eliminate each other when a mean is taken of those of higher and lower strata; and the general form of this curve for the greater mass of the superincumbent atmosphere must be much more constant than that deduced from the thermometer readings of our observatories. Generally, as was assumed by Lamont in his discussion of the problem, the critical phases of the former will be later than those of the latter. This retardation will be greatest where the diurnal range of temperature is greatest, and especially at such intertropical stations as Bombay and Calcutta.

The diurnal march of the temperature at such an observatory as the Colába Observatory at Bombay, must be influenced in a high degree by the local influx of cooler air from the neighbourhood. Situated on a narrow point of land, and surrounded, in all directions but one, by many miles of sea, the atmosphere is scarcely ever calm, and a wind from any quarter other than between north and north-west comes directly from the sea close at hand, the movement of the air increasing with the rise of temperature. To this circumstance, in all probability, it is due that this rise undergoes a slight check at an earlier hour than at any of the other stations. It is very slight, as is shown in the following table, which gives the amount of the rise for

ıch hour from 6 A.M. to noon in each half of the year. But it is ıfficient to explain the early occurrence of its maximum.

Hourly Change of the Forenoon Temperature at Bombay.

| Hour.............. | 6 to 7 | 7 to 8 | 8 to 9 |
|---|---|---|---|
| April to September.. | +0·9 | +1·2 | +1·1 deg. Fahr. |
| October to March .. | +0·5 | +2·1 | +1·9 „ „ |

| Hour.............. | 9 to 10 | 10 to 11 | 11 to noon |
|---|---|---|---|
| April to September.. | +0·9 | +0·8 | +0·7 deg. Fahr. |
| October to March .. | +1·8 | +1·7 | +1·7 „ „ |

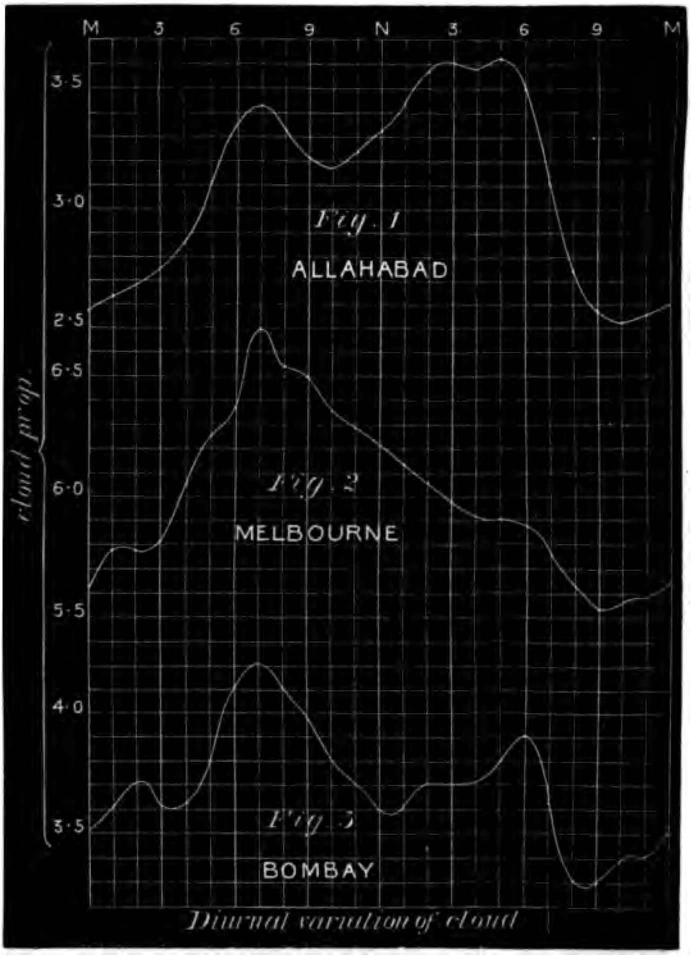
At Calcutta, Batavia, and Melbourne, the observatories are ıfficiently far from the sea to exclude the supposition that they are ıbject to its influence in anything like the same degree as Bombay, ıt at all of these the temperature must be influenced by convection, hich is most active in the summer months; and, as already ımarked, it is at this season that the instant of most rapid heating ·ecedes the barometric maximum by the longest interval. In ırtain of the winter months, viz., August at Batavia, and May and ıly at Melbourne, the time of most rapid heating, and that of the ırometric maximum are as nearly coincident as at Prague and arkand; and in June, at Melbourne, the latter appears to anticipate .e former by about half an hour. Of this opposite anomaly I am ›t prepared with any explanation. More than one circumstance ight be imagined in the local conditions of the observatory which ould retard the instant of greatest rise, but without searching ıquiry and examination on the spot, any suggestion would be mere ıin surmise. I may, however, notice that the June curve of ımperature departs from the ordinary parabolic form in a manner ıat points to the existence of some local irregularity, and that similar regularities are noticeable in other parts of some others of the .onthly curves.

As a final conclusion, if these data, when subjected to the rigorous ıst I have applied, do not give strong support to the hypothesis, either do they, with the single exception just mentioned, show any iscrepancy which is not susceptible of a simple and probable ɛplanation; and the single exception is one which might also robably be explained, were the requisite information available.

### *Evening Maximum.*

The tendency of the skies to clear after sunset in settled weather ıs been noticed by many writers, even in the irregularly variable imates of Europe, and in India it is most striking at all seasons of ıe year. The cloud registers of nearly all stations at which hourly servations have been made, show a strongly marked minimum

between sunset and midnight, the average hour being about 10 P.M.; and some show a second subordinate minimum about 9 or 10 A.M. The cloud curves for Allahabad given by Mr. S. A. Hill on Plate 28, vol. 1 of the 'Indian Meteorological Memoirs,' exhibit both these minima in most months of the year, that of the evening being the absolute minimum of the twenty-four hours. On the average of the year, the mean proportion, at 10 P.M., is 2·52 (on the 0 to 10 scale), that of the twenty-four hours being 3·10: the deficiency therefore is more than one-sixth. The cloud curves of Melbourne given by Dr. Neumayer also show that, in every month except November, the diurnal minimum of cloud is between 7 h. 30 m. P.M. and 1 h. 30 m. A.M.; and, on the mean of the whole year, it occurs at 9 h. 44 m. P.M. At this hour, the mean proportion is 5·55, the general mean of the twenty-four hours being 6·56; so that, here also, the deficiency amounts to one-sixth of the average. At Bombay, the absolute minimum, according to Mr. Chambers's table, occurs at 8 and 9 P.M., and the deficiency is one-ninth of the general average. The mean diurnal cloud curves of Allahabad, Melbourne, and Bombay, for the average of the whole year are given in figs. 1, 2, and 3.




*Diurnal variation of cloud*

Iore striking than any of these is the concurrent evidence afforded the diurnal variation of the Calcutta rainfall. Two series of data e been published; the one based on hourly observations of the occur- ce of rain during twenty-one years,* the other giving the results of n years' records of a self-registering rain-gauge,† which affords surements of the quantities that fell in each hour as well as an meration of rainy hours. The two series are generally accordant, exhibit a diurnal fluctuation of a remarkably pronounced racter. This differs at different seasons of the year, as might eed be anticipated, and it is in the rainy season, when the air is

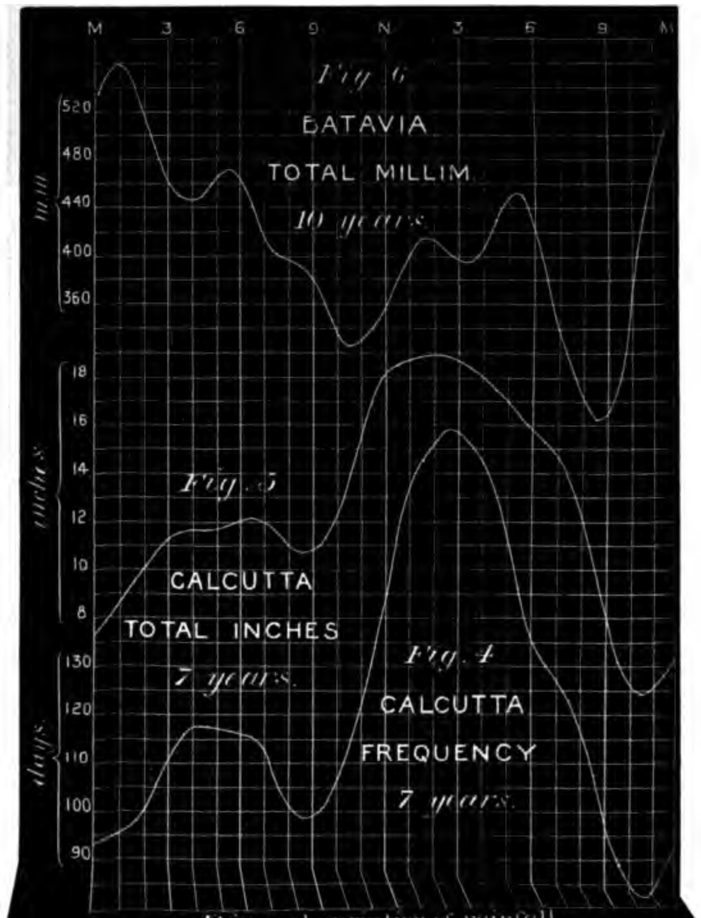
* 'Asiat. Soc. Bengal Journ.,' vol. 48, 1879, Part II, p. 41.

† 'Indian Meteor. Mem.,' vol. 4, p. 43.

nearest to saturation, that the forenoon and late evening minima a most strongly developed. The numerical results of this seaso afforded by both series, and also those of the whole year, are give in the following table, and, in parallel columns, smoothed value obtained by the formula

$$b' = \frac{a + 2b + c}{4},$$

where $a$, $b$, and $c$ are the observed values for any three consecutiv hours, and $b'$ the smoothed value of the middle term. The curve afforded by the latter for the seven-year series are represented b figs. 4 and 5.




*Diurnal variation of rainfall*

| Hours. | In seven years (Alipore). | | | | | | | | In twenty-one years (Chowringhee). Frequency. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Quantities (inches). | | | | Frequency (hours × 3). | | | | | |
| | June to October. | | Year. | | June to October. | | Year. | | June to Oct. | Year. |
| | Obs. | Comp. | Obs. | Comp. | Obs. | Comp. | Obs. | Comp. | Obs. | Obs. |
| Mid. to 1.... | 8·52 | 7·89 | 9·51 | 9·26 | 100 | 94 | 117 | 116 | 298 | 343 |
| 1 „ 2.... | 8·28 | 9·24 | 8·77 | 10·29 | 89 | 96 | 105 | 113 | 327 | 374 |
| 2 „ 3.... | 11·30 | 10·63 | 12·97 | 11·72 | 106 | 105 | 124 | 121 | 348 | 409 |
| 3 „ 4.... | 12·35 | 11·49 | 13·27 | 12·54 | 118 | 115 | 132 | 130 | 353 | 410 |
| 4 „ 5.... | 11·21 | 11·64 | 12·00 | 12·54 | 119 | 117 | 133 | 130 | 364 | 414 |
| 5 „ 6.... | 11·36 | 11·82 | 12·38 | 12·57 | 114 | 116 | 125 | 128 | 376 | 426 |
| 6 „ 7.... | 12·85 | 12·12 | 13·08 | 12·84 | 119 | 115 | 129 | 126 | 373 | 430 |
| 7 „ 8.... | 12·42 | 11·73 | 13·45 | 12·61 | 108 | 106 | 121 | 117 | 373 | 426 |
| 8 „ 9.... | 10·47 | 10·81 | 11·64 | 11·88 | 91 | 98 | 101 | 109 | 357 | 409 |
| 9 „ 10.... | 8·46 | 11·19 | 9·82 | 12·18 | 104 | 102 | 116 | 112 | 410 | 461 |
| 10 „ 11.... | 13·80 | 13·82 | 14·04 | 14·65 | 109 | 114 | 117 | 124 | 458 | 522 |
| 11 „ noon.. | 18·87 | 16·88 | 19·86 | 17·84 | 133 | 134 | 143 | 144 | 505 | 565 |
| Noon „ 13.... | 18·50 | 18·50 | 19·74 | 19·91 | 160 | 156 | 172 | 169 | 543 | 603 |
| 13 „ 14.... | 19·13 | 18·94 | 21·36 | 20·82 | 172 | 172 | 190 | 189 | 537 | 600 |
| 14 „ 15.... | 19·10 | 18·94 | 21·11 | 21·20 | 184 | 178 | 203 | 198 | 572 | 646 |
| 15 „ 16.... | 18·97 | 18·54 | 21·75 | 21·58 | 171 | 176 | 192 | 198 | 477 | 571 |
| 16 „ 17.... | 17·62 | 17·60 | 21·44 | 21·94 | 178 | 167 | 204 | 194 | 464 | 566 |
| 17 „ 18.... | 16·86 | 16·38 | 23·21 | 22·40 | 141 | 146 | 174 | 181 | 413 | 544 |
| 18 „ 19.... | 13·47 | 15·38 | 21·43 | 22·74 | 126 | 130 | 173 | 176 | 397 | 521 |
| 19 „ 20.... | 17·52 | 13·85 | 25·85 | 21·49 | 129 | 123 | 185 | 177 | 343 | 495 |
| 20 „ 21.... | 10·13 | 10·63 | 17·67 | 17·25 | 107 | 107 | 163 | 160 | 321 | 449 |
| 21 „ 22.... | 5·24 | 7·12 | 9·25 | 12·05 | 86 | 89 | 130 | 134 | 261 | 361 |
| 22 „ 23.... | 4·76 | 5·88 | 8·31 | 9·25 | 78 | 82 | 111 | 116 | 282 | 360 |
| 23 „ mid... | 6·48 | 6·53 | 8·67 | 8·77 | 87 | 88 | 114 | 114 | 263 | 319 |

In the rainy season there were 2929 rainy hours in the seven years, giving an average of 122 for each hour of the day. But for the hour between 10 and 11 P.M. there were but 78 instances of rain, or but two-thirds of this average, and from 8 to 9 A.M. but 91 instances of rain, or three-fourths of the average. The deficiency in the quantity of the rainfall was even more striking. The average per hour of the day was 12·81 inches, but the recorded amount for the hour between 10 and 11 P.M. was only 4·76 inches, or less than two-fifths of the general average, and that from 9 to 10 A.M. was 8·46 inches, or little more than two-thirds.

Another equally striking example of the approximate coincidence of interruptions of the rainfall, about the time of the diurnal maxima of pressure, is afforded by Batavia, on the evidence of ten years' registers of the hourly rainfall, published by Dr. Bergsma in the 3rd volume of the 'Batavia Observations.' Here also, it is only the rainy season (December to January) that exhibits this feature in a very decisive manner, and the coincidence is the more remarkable, since, in another respect, the diurnal variation of the rainfall of Batavia stands in marked contrast to that of Calcutta. At Calcutta the greater proportion of the rain falls in the daytime; at Batavia at night. The percentages were respectively as follows:—

| | Calcutta. | Batavia. |
|---|---|---|
| From 6 A.M to 6 P.M. .... | 60·3 per cent. | 47·8 per cent. |
| From 6 P.M. to 6 A.M. .... | 39·7 „ | 52·2 „ |

And the Batavian maximum follows the minimum within four hours, in the proportion of 5 to 2. The following table gives the total rainfall in millimetres, recorded at each hour of the day of the three rainy months during the ten years 1866–1875 (Sunday excepted), and in a parallel column the smoothed values computed as in the former case. The curve, fig. 6, is drawn from these latter figures.

Total Hourly Rainfall at Batavia (December to February), ten years.

| Hours. | Observed. | Computed. | Hours. | Observed. | Computed. |
|---|---|---|---|---|---|
| | mm. | mm. | | mm. | mm. |
| Midn. to 1... | 571 | 550 | Noon to 13... | 364 | 374 |
| 1 „ 2... | 584 | 547 | 13 „ 14... | 423 | 413 |
| 2 „ 3... | 448 | 489 | 14 „ 15... | 413 | 403 |
| 3 „ 4... | 478 | 451 | 15 „ 16... | 362 | 394 |
| 4 „ 5... | 401 | 452 | 16 „ 17... | 439 | 432 |
| 5 „ 6... | 527 | 472 | 17 „ 18... | 486 | 446 |
| 6 „ 7... | 435 | 445 | 18 „ 19... | 373 | 385 |
| 7 „ 8... | 382 | 401 | 19 „ 20... | 307 | 312 |
| 8 „ 9... | 404 | 392 | 20 „ 21... | 263 | 266 |
| 9 „ 10... | 379 | 362 | 21 „ 22... | 231 | 308 |
| 10 „ 11... | 288 | 326 | 22 „ 23... | 508 | 481 |
| 11 „ noon. | 351 | 338 | 23 „ midn. | 475 | 507 |

The general average of all the hours is 412 mm. per hour, but the uantity recorded between 9 and 10 P.M. is only 231 mm., or little ore than half, and that between 10 and 11 A.M. 288 mm., or little ore than three-fifths of this average. It is to be observed that the orenoon minimum of Batavia falls an hour later than that of Calcutta, whereas the evening and principal minimum is an hour arlier. This is exactly what might be expected from the combination of a double diurnal oscillation with one of single period, the latter having its maximum in the former case at night, in the latter in the daytime.

The Melbourne hourly rainfall tables show great variations in different months, and admit of very little definite conclusion, except that, as at Batavia, there is more rain at night than in the day. It is then only in the warm and nearly saturated atmosphere of Bengal nd Java, in their respective rainy seasons, that these diurnal interruptions of the rainfall about the hours of the two barometric axima are decidedly manifested. But in these two cases they are ost marked; and this circumstance, taken in conjunction with the orresponding cloud variation, which is shown by so many stations, oints strongly to a causal connexion between the diurnal variation f pressure and the condensation of atmospheric vapour in the cloudorming strata of the atmosphere, which, I think, we can scarcely fail o recognise.

The mere fact that an increase of atmospheric pressure, from whatver cause arising, is accompanied with a dissipation of cloud and a iminution of rainfall, would not perhaps call for special remark. But it is to be observed that whereas the nocturnal barometric maxium, at all the stations here dealt with, is less pronounced than that f the forenoon, the concomitant effects in the clearing of the atmophere and in the check in the rainfall are much greater in the former ase than in the latter. They seem to point to a forcible compression f the atmosphere, and dynamic heating of the cloud-forming strata. And some such temporary effect does not seem impossible, even at a ime when the earth's surface and the air immediately in contact with t are cooling rapidly. Moreover the temperature curves of Prague, Calcutta, and Batavia all show a very slight irregularity about 10 P.M., which indicates a slight check in the fall of temperature about that our greater than takes place either in the preceding or subsequent hour, and which may possibly be the manifestation of such an action n the lowest atmospheric stratum. Slight as it is, the fact that it ccurs at the same hour in all these curves, and that it coincides with the evening pressure maximum and the strongly marked minima of cloud and rainfall, is at least significant.

*When we tabulate the* differences of the first and second orders of he hourly means of the original observations, at the three stations

specified, it is found that the second difference corresponding to 10 P.M., with a positive sign, has a greater numerical value than either of those preceding and following it, instead of an intermediate value, as would be the case if the fall of temperature after sundown were decreasing uniformly. In the following tables, the figures for Prague and Batavia represent hundredths of a centigrade degree, those for Calcutta hundredths of a Fahrenheit degree. The figures for Calcutta are derived from only six years' autographic traces; those for Prague, apparently from eighteen or twenty years' observations and traces; and those for Batavia from ten years' readings of a standard thermometer. No correction has been applied to the means of the observations as recorded.

Prague (summer).

| Hours, P.M. | 7 to 8 | to 9 | to 10 | to 11 | to mid. |
|---|---|---|---|---|---|
| $\Delta_1$ Change of temperature | −115 | −94 | −85 | −53 | −44 |
| $\Delta_2$ Change of rate of fall | +21 | +9 | +32 | +9 | |

Calcutta (year).

| Hours, P.M. | 5 to 6 | to 7 | to 8 | to 9 | to 10 | to 11 | to mid. |
|---|---|---|---|---|---|---|---|
| $\Delta_1$ Change of temperature | −145 | −248 | −215 | −111 | −87 | −61 | −54 |
| $\Delta_2$ Change of rate of fall | −103 | +33 | +104 | +24 | +26 | +7 | |

Batavia (year).

| Hours, P.M. | 5 to 6 | to 7 | to 8 | to 9 | to 10 | to 11 | to mid. |
|---|---|---|---|---|---|---|---|
| $\Delta_1$ Change of temperature | −79 | −76 | −55 | −41 | −36 | −27 | −27 |
| $\Delta_2$ Change of rate of fall | +3 | +21 | +14 | +5 | +9 | 0 | |

The only further point of some significance, to which I have to draw attention, is that the hour of the evening barometric maximum about coincides with the time when the temperature curve ceases to be strongly concave, and becomes nearly rectilinear, indicating a nearly uniform rate of cooling from that time up to just before sunrise. This fact suggests the possibility that the evening maximum of pressure may be determined by the check in the descent of the cooling and collapsing atmosphere which takes place from 6 or 7 P.M. to about 10 P.M.* But it is very probably combined with other elements,

* This explanation was suggested by Kreil and Espy, and also by myself in a paper read before the Asiatic Society of Bengal in 1876. On it Dr. Sprung remarks:—"Es bleibt aber gänzlich unverständlich, weshalb dieser Effect, schon um 10 Uhr abends, und nicht zur Zeit des Temperatur-Minimums gegen 6 Uhr

hich may be the return of the morning wave of pressure. ɪed unless there be such repetition, it is difficult to understand rise of pressure sets in so early as between 4 and 5 in the ., instead of between 6 and 7 P.M.; that is, after the time when ɜ most rapid. And unless the evening wave is repeated in ɪer, to explain why the morning rise of pressure begins at hours before sunrise.

## Note added August 15, 1888.

the foregoing paper was read before the Society, I have a table of the mean horary readings of the thermometer, at the Surveyor-General's office, Calcutta, (formerly the Observatory) during the same years that have furnished the ic data, quoted in the text, page 415. They have been computed ɜdths of a Fahrenheit degree, and are as follow—(p. 426). ɜtant of the most rapid rise of forenoon temperature computed e figures by the method described in the footnote on page 413 ws in each month :—

| | Max. rise temp. | | Max. bar. | | Interval. | |
|---|---|---|---|---|---|---|
| ary .... | 8 h. | 53 m. | 9 h. | 44 m. | 0 h. | 51 m. |
| uary .... | 8 | 46 | 9 | 52 | 1 | 8 |
| h ...... | 8 | 46 | 9 | 47 | 1 | 1 |
| l ...... | 8 | 22 | 9 | 35 | 1 | 13 |
| ........ | 7 | 54 | 9 | 23 | 1 | 29 |
| ........ | 8 | 2 | 9 | 22 | 1 | 20 |
| ........ | 7 | 55 | 9 | 33 | 1 | 38 |
| ıst .... | 8 | 24 | 9 | 38 | 1 | 14 |
| ember .. | 7 | 41 | 9 | 33 | 1 | 52 |
| ber .... | 7 | 44 | 9 | 25 | 1 | 41 |
| mber .. | 7 | 56 | 9 | 24 | 1 | 28 |
| mber .. | 8 | 56 | 9 | 34 | 0 | 40 |
| Year.... | 8 | 27 | 9 | 35 | 1 | 8 |

riations from month to month shown by this table are, as expected, less than in the table at page 415 computed from only, but the mean interval for the whole year is exactly

regularity of the evening fall of temperature noticed at does not appear in the results of this table, and it must remain doubtful whether its occurrence in the three regis-d in the text is more than a fortuitous coincidence.

treten soll." This objection would be quite valid were the cooling of ıere proceeding at an uniform rate, but not, I think, to the actual facts *ıs above set forth.* This was not noticed in my former communication, *. Sprung refers.*

Mean Values of the Hourly Readings of the Thermometer at Calcutta from 1853 to 1877, recorded daily, Sundays and Public Holidays excepted (deg. F.).

| Hours. | January. | February. | March. | April. | May. | June. | July. | August. | September. | October. | November. | December. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 24...... | 64·13 | 68·97 | 76·02 | 80·12 | 81·61 | 82·43 | 81·75 | 81·49 | 81·56 | 79·25 | 71·80 | 64·44 |
| 1...... | 63·44 | 68·30 | 75·44 | 79·72 | 81·31 | 82·18 | 81·49 | 81·24 | 81·29 | 78·90 | 71·25 | 63·77 |
| 2...... | 62·79 | 67·70 | 74·96 | 79·31 | 81·04 | 81·93 | 81·24 | 81·05 | 81·05 | 78·55 | 70·74 | 63·16 |
| 3...... | 62·21 | 67·18 | 74·45 | 78·97 | 80·89 | 81·70 | 81·00 | 80·80 | 80·85 | 78·27 | 70·20 | 62·55 |
| 4...... | 61·59 | 66·62 | 73·95 | 78·60 | 80·60 | 81·52 | 80·80 | 80·57 | 80·63 | 77·91 | 69·65 | 61·96 |
| 5...... | 61·09 | 66·06 | 73·56 | 78·29 | 80·35 | 81·40 | 80·64 | 80·44 | 80·48 | 77·75 | 69·24 | 61·49 |
| 6...... | 60·65 | 65·60 | 73·14 | 78·14 | 80·49 | 81·53 | 80·68 | 80·40 | 80·38 | 77·52 | 68·84 | 60·96 |
| 7...... | 60·34 | 65·42 | 73·41 | 79·03 | 81·73 | 82·42 | 81·37 | 80·98 | 81·04 | 78·30 | 69·14 | 68·85 |
| 8...... | 62·45 | 67·77 | 76·14 | 81·81 | 84·40 | 84·10 | 82·73 | 82·22 | 82·69 | 80·57 | 72·13 | 63·63 |
| 9...... | 65·96 | 71·21 | 79·21 | 84·81 | 86·92 | 85·79 | 84·03 | 83·58 | 84·04 | 82·29 | 74·92 | 66·86 |
| 10...... | 69·27 | 74·32 | 82·17 | 87·45 | 89·23 | 87·22 | 85·15 | 84·75 | 85·19 | 83·72 | 77·30 | 70·06 |
| 11...... | 72·13 | 77·06 | 84·90 | 89·92 | 91·04 | 88·29 | 86·03 | 85·67 | 86·09 | 84·96 | 79·35 | 72·86 |
| 12...... | 74·35 | 79·32 | 87·03 | 91·53 | 92·34 | 88·96 | 86·59 | 86·18 | 86·74 | 85·77 | 80·76 | 74·89 |
| 13...... | 75·84 | 80·91 | 88·55 | 92·73 | 93·26 | 89·34 | 86·91 | 86·45 | 86·96 | 86·22 | 81·66 | 76·20 |
| 14...... | 76·81 | 81·91 | 89·46 | 93·38 | 93·65 | 89·38 | 86·75 | 86·41 | 86·85 | 86·38 | 82·19 | 77·03 |
| 15...... | 77·15 | 82·40 | 89·86 | 93·38 | 93·48 | 89·13 | 86·40 | 86·14 | 86·35 | 86·30 | 82·00 | 76·86 |
| 16...... | 75·72 | 81·84 | 89·48 | 92·48 | 92·53 | 88·52 | 85·98 | 85·77 | 85·83 | 85·59 | 80·60 | 75·33 |
| 17...... | 74·17 | 80·55 | 87·88 | 90·50 | 90·81 | 87·61 | 85·38 | 85·01 | 84·92 | 84·61 | 79·13 | 73·64 |
| 18...... | 71·56 | 77·60 | 84·84 | 87·68 | 88·31 | 86·30 | 84·47 | 83·97 | 83·82 | 82·83 | 77·04 | 71·11 |
| 19...... | 69·57 | 75·07 | 82·06 | 85·17 | 85·77 | 84·94 | 83·50 | 83·13 | 83·12 | 81·72 | 75·60 | 69·29 |
| 20...... | 68·09 | 73·33 | 80·18 | 83·28 | 84·21 | 84·08 | 82·97 | 82·66 | 82·66 | 80·98 | 74·55 | 67·97 |
| 21...... | 66·90 | 71·98 | 78·72 | 82·12 | 83·15 | 83·45 | 82·60 | 82·36 | 82·30 | 80·40 | 73·63 | 66·85 |
| 22...... | 65·89 | 70·86 | 77·68 | 81·26 | 82·55 | 82·98 | 82·29 | 82·03 | 82·00 | 79·89 | 72·87 | 65·90 |
| 23...... | 65·08 | 70·03 | 76·85 | 80·63 | 81·99 | 82·66 | 81·98 | 81·80 | 81·70 | 70·46 | 72·20 | 65·11 |

'ROONIAN LECTURE.—"On the Origin and the Causation of Vital Movement (*Ueber die Entstehung der vitalen Bewegung*)." By Dr. W. KÜHNE, Professor of Physiology in the University of Heidelberg. Communicated by Professor M. FOSTER, Sec. R.S. Received April 22, Delivered in the Theatre of the Royal Institution May 28, Revised August 15, 1888.

(Translation.)

Among the phenomena of life the movement of masses, or mechanical rork, takes a prominent place. It is the most accessible of all the ital processes to our sensual perceptions, so universally distributed, nd so bound up with most of the activities of organisms, that it iight almost be designated the incarnation of life.

In saying this it must be understood that vital movement is by no eans exclusively confined to animals, that it is not, as was once olieved, a special animal function; on the contrary it is an attribute of *l* living matter, as well of the lowest creatures as of the most highly veloped plants, so that, however extraordinary it may appear, the tivity of our muscles which enables us to transform sensation into tion finds an analogue in the plant. Our conviction of the internnexion and profound unity of all living things has thus a physiogical foundation, based as it is not merely on the community of rivation and of structure of living things, but also on the proof of milar activities.

If a division of the morphological from the physiological is in any ay permissible, it may be said that the unitary conception of life r which our age is distinguished rests in a higher degree on the nowledge of vital *processes* than is commonly recognised, and in fact just as much founded on physiological experience as on that of the *rms* of the organism.

From the traditional conception of life, which scarcely contained ore than that everything between life and death is the antithesis of ie not living, it is a long road we have had to travel to attain to the odern conception of the *real* unity of life; and a remarkable road, nce it bears witness to the confident anticipation of victory, in face all impediments raised up by science itself. Movement, and nothing ss, had been placed at the summit of that antithesis, which physicoiemical research in the animal and vegetable kingdom had revived ith the discovery that the plant transformed kinetic into potential iergy, and the animal the latter into the former. While the animal *ade use of oxygen to* generate heat and perform work through the *tabolism of its substance*, the plant made use of the heat in reducing

2 1 2

and synthetic processes for the accumulation of potential energy in the form of its own consumable substance and the expired oxygen.

With whatever unassailable correctness this conception comprehends life as a whole, affording a pleasing solution of its antithesis by referring animal activities to nourishment by the plant, the latter to the products of the combustion of the animal body, and both in the last instance to the forces of the sun as original source of all life, yet *this* did but cast up the sum total of the processes of life, and did but express more intimately than before that which divides the most highly developed branches of the animal and vegetable kingdom, in which the divergence of forms and arrangements is greatest. For *by the side of* this distinction there exists even between man and the most highly elaborated plant a connexion of a kind quite other than the symbiotic interdependence through the medium of light, air, and food, a community, however, which is not disclosed until we go back to the ultimate elements of organisation.

As in the animal synthetic processes are not wanting, without which it could not even produce a molecule of the colouring matter of its blood, so in the plant we are acquainted with dissociations and combustion, and also with evolution of heat and movement of masses; not that by this I refer to those coarser movements which are referable to turgescence, but primitive movements, which we find first in the smallest elementary organisms, of which all living beings are made up.

We have almost in our own persons lived to see the old anticipation of a single kingdom of living things become gradually an established truth through the discovery of the cell. After the ground-lines of the construction of plants and animals out of originally similar *nucleated* cells had been established by Th. Schwann, and since Darwin's immortal work enabled us to derive everything that ever lived or will live from one single cell, we have come to realise that every single organism renews in itself the work of past ages, and again builds itself up from a germ similar to that from which its most ancient ancestors started.

This conviction has become so firmly implanted in our generation that now we scarcely feel the gaps which still exist in our actual knowledge, and almost unjustly underestimate that which the investigations of our contemporaries yet add to the cell-theory, as if it were mere work of repetition. And yet it has been very extensive and decisive—for example, the recent researches upon the intimate structure of the cell nucleus—since nothing less results from it than that the reproduction of the cell by fission takes place identically, *down to* the most minute details, in all animals and plants (1).*

*Now* if the *shaping* of the cell and all the fashioning of forms is an

* These numerals refer to the reference notes at end.

*activity*, and if Morphology, "since it has made the *arising* of form more its study than the describing of what is already completed," has become part of Physiology, it might be possible and conceivable that research directed to *all* activities and going beyond the *visible* form to the chemical components of the structures and the transformation of substance and force, should observe great differences in processes where all our morphological experience would only have shown identity. We were near enough to this point; for if it were true, as was long assumed, that that which is the bearer and the seat of the most essential of all vital processes *in* the cell is completely formless, it is not easy to see why the form should be so determinant of function.

We have hope that this is not so, and will endeavour to show in Movement the functional as well as the morphological unity of all living matter.

As I have already said, there is an elementary kind of movement in the cell, carried out by the cell-body—that part of the cell which in contradistinction to nucleus, membranes, and various enclosures, has been designated protoplasm. The protoplasm moves itself, as in the case of certain free-living Protozoa, like the long-known Amœba, like the so-called Sarcode—in many cases better comparable to the movement of the pseudopodia of Rhizopods. The resemblance of the latter to what was formerly called the sap-current in many plant-cells, led Ferd. Cohn (2) to interpret plant protoplasm as sarcode, an idea actively supported by Max Schultze (3), the best authority on pseudopodial movement. It is not necessary to say here how widespread protoplasmic movement is, for there cannot be a cell that does not present it at some stage of its existence. Doubt on this subject can only exist in regard to the smallest of all organisms, those of fermentation, of putrefaction, and of pathogenic activity which are too small for observation. But even in these, from the movement they perform as a whole, we have grounds to infer the existence of a protoplasm.

It is proved that protoplasmic movement does not follow external impulses or currents, but is a spontaneous activity. It may go on in opposition to gravity, and overcomes frictional resistance, as shown by the mass itself moving forward on surfaces of every kind, and being able to drag heavy bodies along with it. It is proper mechanical work.

The cause of the movement can only be an internal one, residing in the contractile substance itself, and can only consist of chemical processes taking place within the peculiar pasty, slime-like mass. Yet the question had to be put whether these processes were not first set up by something coming perhaps from the outside, for the movement *changes, sometimes stops* or takes place more slowly, or occurs but partially, *and may by many means* be artificially aroused or diminished

At this point experimental physiological research had to step in, attacking the problem in the same way as it had long before done in the case of the most highly developed contractile structures, the muscles. A muscle behaves so far just like protoplasm that its contraction does work, which can only depend on chemical transformations of its own substance, during which potential is converted into kinetic energy; but it differs in that a distinct impulse from without is needed to set the game going. In normal conditions it receives the initiating impulse from its nerve, and nothing else appears able to take its place, since nothing that might otherwise act upon it, such as the motion of the blood or changes in its constitution, disturbs its repose. But if we let electric currents traverse the muscle, or if we suddenly change its temperature, or act upon it mechanically or chemically, contractions result which do an amount of work out of all relation to the insignificant impulse; the means employed only set going the process peculiar to the muscle, and this is what is meant when we term them *stimuli*, and the faculty of muscles to react to them irritability.

Now is protoplasm irritable in this sense? Experiments on objects of every kind have answered this affirmatively, and more than that have even shown a striking agreement with the irritability of muscle. Of the above mentioned agents, besides rise of temperature, which ultimately sets all contractile cell-substance in maximal contraction—a heat tetanus (4) which disappears with cooling—the electric current has shown itself the most efficient, the stimulus which most surely excites muscles of every kind as well as all nervous matter, and has thence become the most indispensable instrument of physiology.

I may be permitted to adduce an example because it illustrates what is typical and essential (5). It is the case of the fresh water Amœbæ. Every time these organisms, moving like melting and rolling drops, are subjected to an induction shock they contract almost to a sphere, and assume the spherical form completely if the shocks follow each other at short intervals, being by this means fixed for a longer time in this condition. Feebler shocks which singly have no effect, become effective by summation when applied in quick succession, just as in the case of muscle. If the movements of the animal by itself are sluggish, on electrical stimulation they are strengthened and accelerated. Thus the stimulation increases the natural movement, and if increased stimulation brings about repose, it is only the apparent repose of prolonged maximal contraction, like that of our muscles when we hold out a weight for some time at arm's length. All protoplasm behaves in this way from whatever source derived. Larger masses which cannot contract to one sphere (as in many plant cells, or those great cake-like giant masses of the plasmodium of the Myxomycetes) form several such spheres in part

connected by thread-like bridges. Everywhere the taking on of a figure with smallest surface is the result of stimulation, and the expression of augmented contraction (6). That which was outstretched becomes shorter and in like measure thicker, just as a muscle swells when it shortens itself.

Since protoplasm, which either does not move at all spontaneously or so slowly that we cannot perceive it, reacts in the same way to stimuli, we must in the case of ordinary movements infer the existence of processes originating them either in the interior, *i.e.*, automatic stimuli, or of external processes which had at first escaped us. Whoever sees for the first time the action of any one of the simpler independent Protozoa cannot avoid the idea that psychic activity in the strictest sense of the term lies behind it, something like will and design. He sees the elementary being seeking and taking up food, avoiding obstacles, and when touched by foreign objects energetically drawing back, so that he infers sensation also. Possibly he has struck the correct solution, at least we could not refute him, but we should put his deduction to a hard proof if we showed him the same phenomena in the colourless cells of his own blood, or in the protoplasm of a plant-cell; and if we placed before him the rhythmically contracting cells from the beating heart of a bird's egg incubated barely a couple of days, he would certainly wish with us that the search were for a more material cause, and hope that some chemical or physical cause might be found to set up the process. Biology cannot indeed yet claim to have established such causes in explanation of the automatism of protoplasm, but no one will blame the science for continuing the search for them.

Some causes are already excluded, *e.g.*, light, although there are a few micro-organisms whose movements are excited by it (7). Fluctuations of temperature may also be left out of account. On the other hand, oxygen has a notable influence (8). Withdrawal of the vital air stops all protoplasmic movement, though without killing the cell-body, as is seen from the fact that after the loss of automatism electrical stimulation can supply its place, and that the normal movements return on readmitting the air.

We might thus consider oxygen the prime mover in automatism and processes of oxidation its essence, did we not remember that many objects need very prolonged withdrawal of the gas to come completely to rest. This might, however, depend upon the difficulty of removing the last traces of oxygen completely, or it may be that these cannot be removed by the means adopted, but must remain until consumed by the protoplasm itself.

Since protoplasm is of pap-like softness, and may be in a state of *rest or motion at any spot*, its exterior limits are just as capable of *change as everything* within it is capable of quitting its position

taking up any other. Thus the movement cannot become more ordered until obstacles confine and direct it. Between the perfected organisation of contractile substance in muscle and that of protoplasm capable only of unordered movement, we meet a succession of significant steps by means of which we can see how the ordering was attained. The first step would seem to consist in the uncommonly widespread flagellar and ciliary motion, in which an elastic structure, affixed on one side to the contractile mass, is drawn down or bent by its movement, straightening out again in the rhythmic pauses of repose. A further step, at which the contraction can only take place along an axis, consists in the arrangement of the protoplasm in fine strips wholly or partially surrounded by elastic walls, or again in elastic fibrils being embedded in protoplasmic processes. In this case we have actual primitive muscles before us, of which the most elegant examples are known in the Infusoria among the Vorticellæ and Stentores. The movement of these structures is quite like that of muscle. The strips lengthen and thicken, and they may also be contracted in quick twitches or in a prolonged tetanus, the relaxing, like the stage of diminishing energy of all muscles, always proceeding more slowly than that of the increasing energy *before* the maximum.

The muscles of the unicellular Infusoria, no longer doubtful in a physiological sense, show us muscle as a constituent of the cell, and differentiation, without the production of new cells specially endowed for the purpose, taking place in *one* cell to the extent of elaborating contractile elements determinate in form and precise in work. It is very noteworthy that side by side with these muscular strips provided with highly regulated movement, other protoplasm persists, which continues uninterruptedly its ordinary unordered movements, while no such unrest is to be remarked in the muscles. On the contrary. these latter are only used from time to time, apparently for attaining distinct objects. We get the impression that the automatism has, as it were, been lost by this portion, so that it must wait for stimuli to reach it from other parts of the cell. If oxygen really applies the first spur to the protoplasm, it has no direct power over the primitive muscle, so that compared with the protoplasm the muscle is endowed with a diminished irritability.

It has often been said that protoplasm presents the complete set of vital phenomena—assimilation, dissimilation, contractility, automatism, resorption, respiration, and secretion, and even reproduction by dividing. Leaving reproduction on one side, as now disputed and on good grounds, we can assent to the assertion, and examine which of those functions remain for the products of differentiation. In the case of the muscle, we find it to be all of them with the exception of a single one; for, while it undoubtedly takes part in nutrition as in

respiration and carries on a chemical exchange, all of which are indispensable for contractility, *i.e.*, for its work, and since secretion generalised signifies merely the throwing off of broken-down products, it is wanting *only* in automatism, that faculty of reacting to certain stimuli, which remained reserved for protoplasm. In this there is nothing opposed to the assumption that protoplasm as opposed to muscle possesses elementary *nervous* properties.

The above is sufficient to show the transition to the very highly developed motor apparatus, which distinguishes the animal kingdom from almost its lowest stages—I mean the bi-cellular apparatus, which consists of separate cells united only for *one* purpose, one of which presents the exciting nerve, the other the obedient muscle.

From past experience we know that division of the nerve, or more correctly speaking, removal of the nervous cell substance, condemns the muscle to rest. The stimuli then start from the nerve-cell, to them the muscles react by doing work, and they are conveyed to the muscles through the continuation of the cell which the nerve-*fibre* presents. We need not yet trouble ourselves how the excitation of the nerve-cell arises, whether through external—sensory—stimuli, or through an enigmatical psychic act, or through chemical influences; certain it is that these were before the division of the nerve the sole impulse to the muscle's movements. But what the muscles lack we can supply artificially, and more; we can put the nerve-remnant in such manifold states of excitement as it never before experienced from its cell-body, so that the muscle is compelled to undergo many kinds of movement quite new to it, and we can attain the same result by direct stimulation of the muscle.

In the circle of these experiences arose the controversy, not yet quite ended (9), as to muscular irritability, properly the question whether it was, in general, possible to stimulate anything artificially that is not nerve, that is, to set free the activity peculiar to a non-nervous structure by the means at our command.

Haller, who was the first to occupy himself minutely with the stimulation of muscle, and introduced the term irritability, decided, but only incidentally and by the way, that the stimulus could strike also the ramifications of the nerve in the muscle, and he was far from interesting himself in the question in the modern sense, or from suspecting the point of view from which the independent irritability of muscle would later on be questioned. We ought not to blame him much for the latter, since even to-day it is not easy to understand the motives of an opposition now continued for more than a century. At the outset, if I am not mistaken, the teaching of the Animistic, or as it might now be called, the Neuristic school, led to the conception *that not only the excitation* and regulation of the various functions, *ut the actual endowment* of the several tissues with their respective

activities, was the work of that everywhere predominant and distinctly animal contrivance, the nervous system.

In connexion with this, there seems to have arisen the view of the ubiquity of nerves, that is, of so fine a penetration of the parts with nerve radiations that, especially in muscle, not the smallest particle free from nerve could be demonstrated, a view which on the strength of microscopic research is coming up again at the present day in a constantly new dress, and finds energetic adherents (10), but as we shall see is to be refuted, especially by experiment. If we disregard this, we shall find the tendency to consider only nerves as excitable, in some degree founded on the differentiation which transferred automatism to the nervous matter, robbing all the remaining tissues of irritability, so that they only retained the faculty of reacting to the stimulated nerve with which they were bound up. This was as much as saying it was impossible artificially to replace the nervous stimulus, or that if we did succeed, we were strictly imitating it, in which case, indeed, we should have come unawares upon the solution of the problem of motor innervation. Against such arguments it availed nothing to point out the excitability of nerveless sarcode, as was often done in favour of irritability: for, just as it was formerly useless, because the real genetic connexion of sarcode and muscle was not known, so to-day it would have to be rejected, because automatic protoplasm can also be correctly considered nervous.

A non-irritable muscle would strike us as strange enough, and, against all expectation, different from the nerve, when we consider that the nerve-fibre, although incapable of being affected by all the natural stimuli which excite its ganglion cells, free that is from automatism, is artificially excitable at every spot by the most different agents. However, we have no further need of such considerations, since the question of irritability lies within a region where instead of speculation, observation and experiment have become decisive.

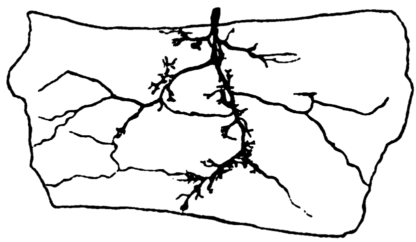
As a matter of fact, the older statements, long considered a good basis for opposing irritability, are incorrect, as for instance, that an excised piece of muscle in which no nerves could be seen with the lens did not twitch on stimulating it.

We can show you a little piece 3 mm. long from the end of the sartorius muscle of the frog, in which the best microscope discovers no traces of nerves, easily made recognisable by osmium-gold staining (fig. 1). Such a piece, transversely cut off, twitches as we know at each effective muscular stimulus. Pieces which can be obtained free from nerves from many other muscles, behave in the same way, as for instance pieces from the delicate muscles of the pectoral skin of a frog (fig. 2).

Further, the assertion was incorrect that everything that excited the nerve made the muscle twitch, and vice versâ; for we see here a

FIG. 1.

FIG. 2.



sartorius suspended in ammonia vapour, contracting powerfully, while a nerve entirely submerged in liquid ammonia appears wholly unstimulated, for it does not rouse the thigh muscles from their repose. (Experiment shown.)

Conversely, we see a thigh whose nerve dips into glycerine in maximal contraction, and on the other hand, a muscle in contact at its excitable end with the same glycerine remains at rest, yet it twitches if I dip it in up to its nerve-bearing tracts (11).

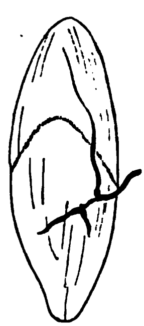
These are old experiments (12), and it is admitted they have overthrown the earlier opinion. But they have not been deemed sufficient to prove muscular irritability, because the ultimate endings of the nerves might have an irritability other than that of their stems. This is the only objection still raised. One could wish no other were conceivable, for this one admits of refutation.

To this end permit me to go a little into detail concerning nerves. Nerves are processes of nerve-cells composed of fibrils of immeasurable fineness, which in the so-called axis cylinder of the medullated nerves are united by a stroma inside a very fine membrane called the axolemma. In proportion to the microscopic dimensions of the ganglion cells of which the separate nerve-fibres form a part, these latter are for the most part enormously long, many as long as our arms and legs, and that is one of the reasons why the perception of the unicellular nature of the nerves made way but slowly. In fact it was not easy to accustom oneself amid the microscopic swarm of cells, to find single ones so grown in length that they could be wound about us like a cocoon thread. As it is the task and function of the motor nerves to lead towards the periphery the impulses sent out by *their ganglion cells in the* spinal cord, their activity always admits of *ready perception* through the muscular twitching. Even when the

nerve is divided and artificially excited at the peripheral end, the muscles betray it. On the other hand, no visible physiological reaction is found at the central origin of the motor fibre when stimulated at the periphery, so that at first we were quite in darkness as to whether in general it conducted centripetally. Nature, however, has presented us with a contrivance by which we are enabled to demonstrate the possibility of such an inverted or centripetal nerve-conduction. The contrivance consists in the branching division of nerve-fibres so frequently found in muscles, as will at once be seen in a preparation from a frog (fig. 3). In many muscles these branchings are so arranged that we can use them for an experiment as simple as it is conclusive of nerve-conduction in both directions.

FIG. 3.

FIG. 4.



In the *gracilis* muscle of the frog the nervation is fashioned in the manner displayed schematically upon this diagram (fig. 4) and in more detail on the following (fig. 5). In reality the arrangement is like this. Now, if I cut up the muscle according to this diagram (fig. 6), we get at the tip Z nerve-fibres which are connected with the muscle-fibres at O and U only by the branchings at the points *xx*, but which in life served only for the parts of the muscle removed at *f* and *f'*.

An experiment (13), viz., the stimulation of *z* (fig. 6), will now convince you that nerves severed from their own muscle-fibres act quite well backwards upon those placed centripetal to them, which they can only do if nerves can also conduct centripetally, and so long as a path is preserved for this through the branchings. If we cut out the neighbourhood of the branchings it is all over with the reaction of the muscle.

We can make another experiment on the same muscle (14). We see that when we excite the lower tip of the muscle, only the lower portion twitches and not the upper. The two portions are in fact

FIG. 5.

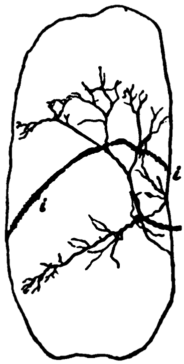
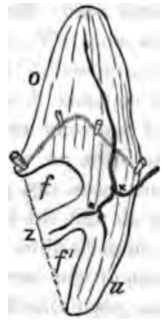



FIG. 6.



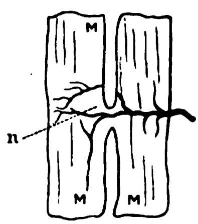

connected only by means of a very short tendon, the so-called *inscription*, which passes completely through the muscle (*ii* in fig. 5), so that it really consists of two muscles. If the nerve common to both is stimulated at any point, then both parts of the muscle contract, but if the muscle substance itself is stimulated, then the contraction travels no further from the place where the stimulus was applied than to the limits of continuity of the muscle-fibres.

The power of motor nerves to conduct in both directions is certainly of general significance in regard to the inner mechanism of nerves, but we have only approached it here, because it was necessary for the decisive proof of muscular irritability, as obtained in our last experiment with the *m. gracilis.* Whenever a muscle is provided with a nervation and branchings of the separate nerve-fibres like that of the *gracilis*, some group of muscle-fibres can serve to indicate whether a stimulus has affected this alone or the nerves lying in it as well. If nerves are present at the point of stimulation, and if the agent was at the same time a nerve stimulus, this is shown by the simultaneous contraction of distant parts which are accessible by means of the nerve's power of conducting in both directions. In cases where we can see the coarser nervation, the indirectly produced contractions can be predicted, and these form so certain a criterion of neuro-muscular excitations that by them the presence of the finest nerves may be proved, whose existence might otherwise be quite incapable of proof by any other means, as, for instance, by the use of the microscope. If these contractions are wanting, as was the case in our experiments with the lower end of the muscle, we know that either the spot stimulated is free from nerves, or that the stimulus employed was ineffectual as to the nerves and affected the muscle substance *exclusively. In both cases then independent irritability is proved for those muscle-fibres which were directly excited and contracted.*

Now since we have just employed an electric stimulus which is equally effectual on muscle and nerve, it follows that we had to do with the first case; that is to say, the muscle showed itself free from nerve at its end. We have reason for specially bringing forward this experimental proof of the absence of any kind of nerves in large tracts of muscle, because it compels those who in spite of all assume the presence of nervous matter in certain microscopic disks and striæ of the muscle-fibre as a whole, to deny that this supposed nervous element possesses any power of conducting in both directions or any irritability at all; for in fact it is not possible to excite the motor nerve of a muscle-fibre by any stimulus whatever applied to the actual terminations of the nerve within the fibre. The facts besides combine to prove, as need hardly be said, yet another proposition—they prove at the same time that pure muscular excitation does not travel back to the nerves.

This may be shown still better with the small pectoral muscles of the frog's skin than with the *m. gracilis*. We need only dissect it in

FIG. 7.

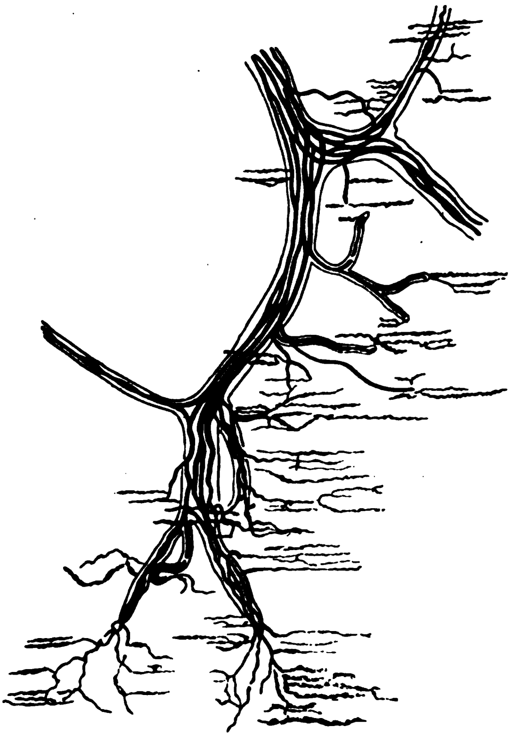



the manner shown in the drawing (fig. 7) and stimulate the spots *n* and M; if we stimulate *n* everything contracts, if M the excited half only.

The preparation which you now see (corresponding to fig. 2), and which shows the nervation of the very thin muscle with all the nerve-endings stained dark with gold, makes that relation clear, for here again in truth the result of morphological research is in gratifying accordance with results obtained experimentally. The muscle is seen to be for the most part free from nerves; indeed the entire nervation with all the nerve-endings might be said to be formed of one nerve *line* only, if we disregard the few digressing fibres which again in part are not motor.

Under rather higher powers we see the nerve-endings proper (fig. 8), the distinct demonstration of which by means of the gold method has now been achieved, in much the same way as here, in all the classes of vertebrates with the exception of the osseous fishes. In all cases these decisive preparations have proved that the vastly preponderant number of the muscle-fibres is entirely free of nerves, and that the

Fig. 8.



nerve-endings are confined to very small spots which we term fields of innervation. Most muscle-fibres have only one field of innervation, very long ones occasionally several, at the most eight. Thus the assumption, opposed to the idea of independent irritability, that muscle substance is well-nigh completely riddled with nerves, is refuted and rejected from the morphological side also.

From the absence of nerves in long tracts of muscle-fibre we immediately conclude that the latter shares with nerves the faculty of independently propagating its own excitation. This is what the beautiful microscopic observations of Sir William Bowman (15) on insects' muscles long since led us to suspect. As in the nerve so in the muscle, conduction takes place in every direction, and as the field of innervation almost without exception occupies a median position during a *normal contraction*, the conduction takes place in both directions, *towards the tendinous* ends. By way of distinction the velocity of

conduction is, according to species, temperature, &c., three to ten times less than von Helmholtz fixed it for nerve. As conduction in irritable tissues means nothing else than that one excited spot becomes the stimulus for the adjoining portion at rest, the independent irritability of the muscle-fibre comes into employment in every movement and during the entire duration of life; from the moment that the field of innervation becomes active all the muscle substance remains left to itself, and until the contraction is ended must be regarded as independent and acting in response to its own direct excitation.

Once clear on the fundamental question, and sure as to the method we have to employ in order to stimulate according to choice either muscle or nerve-substance alone, or both together, we may seek to determine in what respect the irritability of the two components of the motor machine differs. The differences as regards chemical stimulation appear very great; in respect of electric, thermic, and mechanical, on the other hand, only quantitative. However, under chemical stimulation, according to Hering's classical researches (16), a point formerly overlooked comes into consideration, namely, the complication introduced by the electromotive behaviour of the tissue, an automatic electrical stimulation one might say. When stimulation takes place by moistening the transverse section with conducting liquids, it is indeed difficult, if not impossible, to trace the chemical factor in presence of the electrical. Gaseous stimuli alone, like ammonia, have thus far remained free from the suspicion of acting electrically. To these a few others of similar action, such as bisulphide of carbon (17), have been added, and such as are conveyed to the muscle by the blood-vessels, and bathe the fibres from all sides. With these in particular we may class distilled water, which is excessively destructive to irritable substances, von Wittich (18) being the first who showed how strongly it stimulates muscles, while killing nerves without excitation. But, again, with this kind of stimuli, we cannot at present tell whether they do not set up in the tissues, over narrow but numerous areas, excitatory electric currents, thus working only indirectly by way of auto-electric stimulation. And since, finally, the same might apply to the thermic and mechanical actions which likewise arouse demarcation currents in the muscle, that is, to all stimuli, we find ourselves in the presence of the possibility of reducing all irritability to a reaction to electrical processes, and of seeing vital electricity elevated into immeasurable importance.

The means by which muscle may be stimulated interests us, in the first place, on this account—to ascertain, once for all, how it procures *its excitation in life*, or what may be the action of nerve upon it. Did *we know that*, we should have grasped at the same time the nature of *nervous* activity.

Nerves end blindly in the muscles; as a rule they are not even finely pointed, and still less do they spread out diffusely in such a way as might make the true ending difficult to find. They end quite distinctly. But the ends always lie beneath the sarcolemma, in such a way that no foreign tissue intrudes between them and the muscle, so that what is fluid in the muscle can directly moisten the nerve. The sublemmal nerve is clothed with nothing else than the axolemma. The nerve never penetrates into the depths of the muscle substance; on the contrary, it remains confined to the sublemmal surface of the contractile cylinder or prism. Each nerve end consists of several branches, like antlers, arising by division, which together form the terminal nerve-branch. Apart from the form of the antlers, this short description is exhaustive for many animals, since neither in the sublemmal nerve need any special additional structures occur, such as nuclei, nor any kind of modification of the muscle substance in the field of innervation. There is much to indicate that the nerve-fibre proper, or axis-cylinder, does not change its constitution in passing through the sarcolemma, still it is to be remarked that the twigs of the terminal branches, although as long as they live often apparently longitudinally striated, have not yet, even in the most favourable stainings, been found to present the general fibrillar structure of nerves.

According to these results of morphological research, it appears that contact of the muscle substance with the non-medullated nerve suffices to allow the transfer of the excitation from the latter to the former. The only strange thing is that in reversed order excitation of the muscle never extends to its own nerve. This is still stranger because, according to Matteucci's well-known discovery, a *foreign* medullated nerve simply laid upon the muscle is powerfully excited by the contraction—so powerfully that the smallest contracting muscle barely touching it in more than a mere point excites the strongest nerve, while, on the other hand, we never see muscles excited by nerves which are merely pressed against them.

In the investments, then, of the nerve and the muscle substance appears to exist one of the elements which admits the neuro-muscular excitation *exclusively* to the field of innervation, and among those investments it need not be the medullary sheath. The delicate membranes of the sarcolemma and neurilemma suffice, for muscle cannot be excited by superimposed *non*-medullated nerves. At any rate, I have tried in vain to excite muscles by the most intimate contact of the fine terminal ramification of the optic nerve in the retina or the *n. olfactorius* from the pike, or even the delicate nerves of Anodonta, by stimulating these non-medullated nerves.

*If we imagine the* activity of the nerve to start with a chemical *process, and that a* chemical stimulant, as du Bois-Reymond (19) on

suggested, is, at the same time, secreted in contact with the muscle, we understand very well the necessity of direct contact, and in this case it would suffice if the sublemmal nerve were to run in *any* form for a short distance under the sarcolemma. The branching then would mean the enlarging of the contact. But however rich and intricate the ramifications may be, we can by no means say they display throughout the principle of increase of superficies; on the contrary, they are often astonishingly poor and small. As concerns their form, they are *not* irregular, but so strikingly uniform that this point deserves particular attention as being apparently indispensable for innervation.

Instead of describing the forms, allow me to show you the object itself in a selection taken from the most diverse vertebrates. First from the Amphibia (fig. 9): rod-like branchings with long outstretched twigs, a form which crops up again in a remarkable way in many birds. The rule here is asymmetry of the divisions: all the twigs have the form of a bayonet.

FIG. 9.

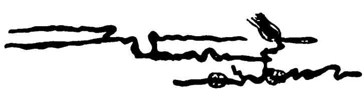


FIG. 10.

The following preparation shows the termination in the dog (fig. 10). Here the branches are crooked, and hence quite divergent, so that the points of agreement with the form of the Amphibia are at first overlooked. But if we examine the divisions, you will remark that these are again unsymmetrical and give off branches whose ends lie very diversely removed from the common place of origin. The ends are, as a rule, turned towards each other, and often so approximated that it is at times troublesome to find the gaps between them, and if they do not lie in the same plane they appear to be united into a ring. In other cases one end overlaps the other, but we then find that all the points of the branches which are turned towards each other lie at unequal distances from the nearest bifurcation. This law holds good in all the thousand cases of motor endings thus far observed and shows a strict order in the apparent chaos of these structures. And yet among the organic forms there is scarcely one which varies so much in other respects and often is so inextricably complicated as this.

The drawings (fig. 11, from the muscles of the guinea-pig, and fig. 12 of the rat) and a preparation from a lizard (fig. 13) may serve

FIG. 11.

FIG. 12.

FIG. 13.

ı a voucher for the truth of the above statement. We see there rerywhere the hooks making their appearance with a short and a ng claw, like the swivel we hang our watch on in the pocket.

The voluntary muscles of all vertebrates and of many invertebrates ›nsist of fibres, the contents of which are perfectly regularly dis- ›sed in layers and transversely striped. For shortness, this striped ass may be called "rhabdia." This it is which has been universally entified with the contractile substance. But it has been ascertained ıat in many cases the nerve-ending does not come at all into direct ›ntact with the rhabdia, but with another mass, which is highly ıcleated and of pap-like softness. This latter is unstriped, and has l the appearance of protoplasm. It occurs in very varying quantity ıder the nerve-antler; in Amphibia, where the sublemmal nerves ın out in a long course, it is not apparent as a separate layer, but it :curs more abundantly in the same measure as the branchings retract, ıd the field of innervation becomes smaller. At first it is found ıiefly between the twigs, in the intervals of the branching, and then the form of a "sole," which among the much contorted branchings reptiles and mammals grows thicker, till it sometimes in some nerve ninences forms quite a thick cushion. Since we have succeeded in aking the nerve-endings visible in uninterrupted series of very fine ctions of mammalian muscle stained with gold, there can no longer › any doubt that the complete separation of the sublemmal nerves om the rhabdia by measurable layers of sole-protoplasm, though ›t the rule, is yet by no means rare, and that many muscles possess › other sort of nerve-endings than such as these with apparently direct contact (20).

It would be difficult to understand why the innervation should ıve in some muscles, as in the Amphibia, no intermediate layer while ıving in the majority of cases an interrupted layer, and in others continuous layer of varying thickness to traverse. But when we ›nsider *what the substance* of the sole is, of what it consists, how it *distributed, and* when we know its origin, it appears that it i-

identical and stands in continuous connexion with the long-known second constituent of muscle-fibres, of which as well as of the rhabdia the fibres are composed. It is that substance, considered by Max Schultze to be the protoplasmic remnant of the cells composing muscle, which occurs in greatest amount around the nuclei of muscle, and extends in long threads throughout the entire muscle-fibre. So many transverse connexions occur on the very numerous stronger and finer nucleated threads that the whole mass, called sarcoglia, becomes a trellis-work almost of the same fineness as the better known transverse striation of the rhabdia, and everywhere surrounds and interpenetrates the latter. This minute internal structure of muscle has only become at all well known since the introduction of gold staining, thanks especially to Messrs. Retzius and Rollett (21). Had it been suspected earlier, and had we appreciated the volume of the sarcoglia whose existence is thereby shown and which rivals that of the rhabdia, we might have studied this component of muscle in its physiological relations to contractility, as well as in its morphological and genetic relations which are the only ones yet known.

If now in many cases it appears that the nerve comes in contact only with the surface of a thick layer of sarcoglia, while the rhabdia everywhere is covered by very fine layers of the latter, whose absolute absence in the field of innervation can nowhere be demonstrated, we have to conclude that in general the nerve does not act directly upon the rhabdia, but only on the sarcoglia. This at once gives the latter a physiological interest. We have to ask whether the glia is the medium that conducts the stimulus between nerve and rhabdia, or whether it is itself the contractile element while the rhabdia has a signification other than that formerly attributed to it when we were completely ignorant of the glia.

All contractile substance requires the co-operation of an elastic element. Where is this to be found in the muscle-fibre? The envelope of sarcolemma which is certainly elastic but delicate, and whose mass is almost infinitesimal compared with that of the muscle-fibre, cannot satisfy the requirement; but more solid structures freely distributed in the paste-like sarcoglia could perhaps do so, and such we find in the rhabdia, in the form of prismatic particles ranged with such constancy and with such regularity longitudinally and transversely, that we may hold them to be the elastic element. Then the sarcoglia would become the contractile element, and the nerve would have an easier task.

I could wish that this view might be accepted as an hypothesis. As far as I can see it does not contradict experience, for it only puts *back the* muscle nearer to the protoplasm and to all that is contractile, and so far coincides with experience that we find muscles in *the same* measure less elastic and more sluggish in protoplasmic

ovement the richer they are in sarcoglia, as in the case of the d muscles, nucleated and rich in glia, which contract more slowly ıt with greater power than the white muscles poorer in glia which e quick and spring-like, and also the sluggish embryo muscles, in hich glia predominates because as yet but little protoplasm has en converted into rhabdia; and further the cells of unstriped uscle-fibre, which are wanting in the regular transverse striation, ıd contain, as it appears, besides more abundant glia, an elastic aterial of special form and arrangement.

The hypothesis would be overthrown if contractile fibrils were und in which no sarcoglia was to be detected. But even in the finest rils of *Stentor*, the structure of which Bütschli (22) has recently ucidated, we must hold the significance of punctated transversely netrating indentations to be protoplasmic, and we can therefore arcely expect ever to find a contractile thread in which nothing hatever should be found of the primitive contractile material such it everywhere exists.

Of late this view (23) has been defended from the purely morphogical side (24), on the strength, namely, of the very fine reticular ructure of protoplasm to which more attention is being paid, and hich is demonstrable on objects of all grades of organisation. Protoasm, in fact, is not so formless as at first appeared, but shows a ructure comparable with nothing better than with the appearance esented by a transverse section of muscle with its glia framework ained with gold. We may expect that these reticular structures, whose nsistency appears to vary extraordinarily, will some day lead to the tablishment of a fruitful hypothesis of the inner mechanism of otoplasmic movement, in place of that held hitherto which affords glimpse into the essence of vital mechanical work.

Compared with this larger problem, that of the causation of vital ovement appears the more accessible of the two, the latter being conlered as a physiological inquiry after the constitution of the normal imulus by which work is done. Perhaps, indeed, the answer is to looked for from the most perfected organisation of muscle, where e initiatory process is localised by a distinct nerve-ending, rather an from the primitive organisation where the excitation may t in at any place, and lies in the protoplasm itself. We know disıctly that the muscle-wave begins in the field of innervation, for have long seen the natural contraction in the interior of transrent insect larvæ starting from the nerve eminences. We know is also from the experiments of Aeby, who followed the muscle-wave yographically from the nerve-line onward, and now we are able to splay the beginnings of the contraction as local thickenings at the *int of attachment of* the nerves caught and fixed by sudden harden-. *Since the nerve* grasps the muscle in a restricted region

expends its action upon this exclusively; that which follows on as muscular activity is the nerve's work no longer.

Galvani and his successors for more than a century suspected that nervous forces were electrical, and, in reality, the celebrated champion of electro-physiology in our day has been able with the galvanometer to render the excitation of nerves, unattached to muscles or ganglion-cells, evident as the negative variation of the natural nerve-current, to cause movement of a magnetic needle instead of a muscle, or to put the needle in the place of sensation. After this no consideration of the nature of nervous activity is conceivable which does not take into consideration this discovery of du Bois-Reymond's—least of all where the nerve has to excite something with which it is not fused, like muscle, but which it only touches, and that not directly, while still invested by the axolemma. Only during excitation, as Ludimar Hermann has taught us, are electric currents issuing from the nerve through its conducting surroundings, in which the course of these currents of action is to be estimated from the duration of the negativity of the nerve-tract excited, and from the speed of propagation of the nerve-wave, if we know the conductor and the disposition of the nerve. The motor ending fixes the latter, and so peculiarly that we can only presuppose from it a furthering of the excitor effects of the currents of action.

The currents of action of muscle, whose electromotive behaviour agrees so wonderfully with that of nerve, have long been proved to produce excitor effects, although only powerful enough to act upon nerves; but there are also, under certain conditions discovered by Hering, such effects from nerve to nerve (25). Is the possibility, we may hence ask, to be excluded of one muscle exciting another, and is it quite impossible that a nerve only throws a muscle into contraction by means of its currents of action?

The first question we can answer. I will do so by a simple experiment. Two muscles, the nerves of which are disposed of by poisoning with curare, need only to be pressed together transversely over a narrow area to make a single muscle of them of double length, in which the stimulation and contraction are propagated from one end to the other. Since the transference from one muscle to the other is done away with as soon as we bring the finest gutta-percha between the muscles as an insulator, or gold-leaf as a secondary circuit, the first muscle must have excited the second electrically (26).

NOTES.

1. The most complete exposition of these important later discoveries on the *reproduction* of the cell is to be found in the book of W. Flemming, '*Zellsubstanz, Kern* und Zelltheilung,' Leipzig, 1882. *Cf.* the "Kurze historische Übersicht" *(p. 385)*, with the quotations from the works of Schneider, Strassburger, Bütschli,

Flemming, O. Hertwig, and the researches of Auerbach, Balbiani, van Beneden, Eberth, Schleicher, Balfour, and others.

2. Ferd. Cohn: "Nachträge zur Naturgeschichte des Protococcus pluviatilis." 'Nova Acta Acad. Leopold. Cæsar.,' vol. 22, P. II, p. 605 (1850).

3. Max Schultze. 'Ueber den Organismus der Polythalamien.' Leipzig, 1854.

4. W. Kühne: 'Untersuchungen über das Protoplasma und die Contraktilität.' Leipzig, 1864, pp. 42, 66, 87, 102.

5. Kühne: *ibid.*, p. 30.

6. Th. W. Engelmann five years later confirmed the passage of protoplasm, especially of Amœba, to the spherical form on stimulating; *cf.* his "Beiträge zur Physiologie des Protoplasmas," 'Pflüger, Archiv,' vol. 2, 1869, p. 315, and 'Handbuch der Physiologie, herausg. von L. Hermann,' vol. 1, p. 367.

7. Engelmann: "Ueber die Reizung des contraktilen Protoplasma durch plötzliche Beleuchtung." 'Pflüger, Archiv,' vol. 19, p. 1.

8. Kühne, *l.c.*, pp. 50, 67, 88–89, 104–106. The cessation of the so-called sap-stream in the cells of *Chara* on excluding the air by oil was observed as far back as 1774 by Bonaventura Corti; and further by Hofmeister in *Nitella* under the influence of reduced atmospheric pressure. *Cf.* Engelmann in 'Handbuch der Physiol., von Hermann,' vol. 1, Part 1, p. 362.

9. *Cf.* J. Rosenthal: 'Allgemeine Physiologie der Muskeln und Nerven.' Leipzig, 1877; p. 255.

10. J. Gerlach: "Ueber das Verhalten der Nerven in den quergestreiften Muskelfäden der Wirbelthiere." 'Erlangen, Phys. Med. Soc. Sitzber.,' 1873.—'Das Verhältniss der Nerven zu den willkürlichen Muskeln der Wirbelthiere.' Leipzig, 1874.—"Ueber das Verhältniss der nervösen und contraktilen Substanz des quergestreiften Muskels." 'Archiv Mikrosk. Anat.,' vol. 13, p. 399.

A. Foettinger: "Sur les terminaisons des nerfs dans les muscles des insectes." 'Archives de Biol.,' vol. 1, 1880.

Engelmann: 'Pflüger, Archiv,' vol. 7, 1873, p. 47; vol. 11, 1875, p. 463; vol. 26, p. 531.

In these publications it is sought to prove that the motor nerves pass either into the interstitial nucleated substance of the muscle (therefore into the sarcoglia) or into the layers of the "Nebenscheiben." This latter view is opposed by, among others, A. Rollett in his thoroughgoing exposition of the structure of muscle (Vienna, 'Denkschriften der k. Akad.,' vol. 49, p. 29), and W. Kühne ('Zeitschr. f. Biol.,' vol. 23, p. 1).

11. The experiments were performed during the lecture by projecting on the wall images of the preparations enlarged some thirty times.

12. Kühne: "Ueber direkte und indirekte Muskelreizung mittelst chemischer Agentien." 'Müller's Archiv f. Anat.,' 1859, p. 213.

13. Kühne: "Ueber das doppelsinnige Leitungsvermögen der Nerven." 'Zeitschr. f. Biol.,' vol. 22, p. 305. To demonstrate the experiment on the *gracilis*, the muscle was fixed on a white piece of cork by needles, and held by elastic holders, and its image thrown on the wall highly magnified by a Krüss lantern.

14. Kühne: *ibid.*, pp. 312, 324.

15. William Bowman: "On the Minute Structure and Movements of Voluntary Muscle." 'Phil. Trans.,' 1840, p. 457; and "Muscle—Muscular Motion" in the 'Cyclopædia of Anatomy and Physiology,' edited by B. B. Todd, vol. 3, 1847, pp. 506–530.

16. E. Hering: "Ueber direkte Muskelreizung durch den Muskelstrom." Vienna, '*Sitzber. k. Akad.*,' vol. 79, Abth. 3, 1879.

17. "*Ueber chemische* Reizungen; nach Versuchen von stud. med. C. Iani." '*Untersuch. aus der Physiol.* Instit. der Univ. Heidelberg,' vol. 4, 1882, p. 266.

18. v. Wittich: 'Experimenta quaedam ad Halleri doctrinam de musculorum irritabilitate probandam instituta.' Königsberg, 1857, and 'Virchow, Archiv,' vol. 13, 1858, p. 421. In these papers, with the discovery of the excitation of muscle by distilled water, appears without doubt the first fact which overthrew the old theory of the equal irritability of muscle and nerve.

19. E. du Bois-Reymond: 'Gesammelte Abhandlungen zur allgemeinen Muskel- und Nervenphysiologie;' vol. 2, p. 700.

20. Kühne: 'Verhandlungen des Naturhist.-medicinischen Vereins zu Heidelberg;' Neue Folge, vol. 4, pp. 4, 5.

21. G. Retzius: 'Biologische Untersuchungen,' 1881.

A. Rollett: "Untersuchungen über den Bau der quergestreiften Muskelfaser." 'Wien, Akad. Denkschr.,' vol. 49, 1885.

22. 'Dr. H. G. Bronn's Classen und Ordnungen des Thierreiches, neu bearbeitet von O. Bütschli.' Leipzig und Heidelberg, 1888, vol. 1, p. 1298.

23. Kühne: "Neue Untersuchungen über motorische Nervenendigung." 'Zeitschr. Biol.,' vol. 23, pp. 88–95.

24. A. van Gehuchten: "Étude sur la structure intime de la cellule musculaire striée." 'La Cellule,' vol. 2, p. 289.

25. E. Hering: 'Sitzber. der k. Akad. zu Wien,' vol. 85, Abth. 3, 1882, p. 237.

26. Kühne: "Secundäre Erregung vom Muskel zum Muskel." 'Zeitschr. Biol.,' vol. 24, p. 383.

The drawings, figs. 1, 2, 3, 5, 8 are taken from the papers of Dr. K. Mays: "Histophysiologische Untersuchungen über die Verbreitung der Nerven in den Muskeln" ('Zeitschr. Biol.,' vol. 20, p. 449), and "Ueber Nervenfasertheilungen in den Nervenstämmen der Froschmuskeln" ('Zeitschr. Biol.,' vol. 22, p. 354); figs. 9—13, from the author's work in 'Zeitschr. Biol.,' vol. 23, pp. 1—148, Plates A—Q.

## "Contributions to the Chemistry of Chlorophyll. No. III." By EDWARD SCHUNCK, F.R.S. Received June 19,—Read June 21, 1888.

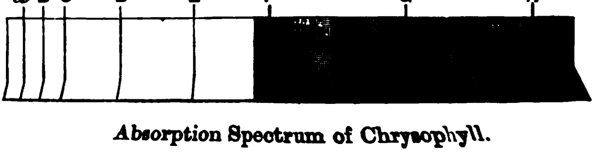
*Products of the Action of Alkalis on Phyllocyanin.*—In the first part of this memoir I gave a general account of the action of alkalis on phyllocyanin ('Proceedings,' vol. 39, p. 355). I shall now proceed to give the results obtained on further examining the products due to this action. The description of the products appearing in the first stage of the process of change induced by alkalis forms the subject of the present communication.

The great trouble involved in preparing any considerable quantity of phyllocyanin in a state of purity made it desirable to find out a method, if possible, of obtaining the products of the action of alkali directly from chlorophyll itself. The object in view was attained by acting on chlorophyll first with alkali and then with acid, thus reversing the process previously adopted and at the same time leading to the discovery of several new and interesting compounds, the formation of which had not been anticipated.

he plan I have pursued is as follows:—Fresh leaves—I prefer s with broad blades to any other material—are exhausted with ng spirits of wine containing from 80—82 per cent. of alcohol. green extract is filtered hot, and being allowed to stand for a or two away from the light, yields a dark-green voluminous ›sit, containing chlorophyll mixed with fatty and other matters. deposit is filtered off for further treatment, the pale-green ıte being rejected. The green mass on the filter is now to be ted with a boiling solution of caustic soda in strong alcohol, :h dissolves it in part. The insoluble portion is filtered off, and r washing with alcohol appears almost white.* Through the dark- n filtrate a current of hydrochloric acid gas is then passed until quires a strong acid reaction. The liquid first becomes yellowish- n, but after some time the colour changes to a dull purplish- n, and small crystalline needles arranged in stars, purple by cted and dull-green by transmitted light, begin to appear on the

Minute sparkling red crystals are always found interspersed in the amorphous of which the residue left by alcohol for the most part consists. These crystals he chrysophyll of Hartsen, the erythrophyll of Bougarel, a very beautiful ance, which may be freed from the impurities accompanying it in this case in ›llowing manner:—The residue, after washing with alcohol, is treated in the with chloroform, which dissolves the chrysophyll, leaving the greater part of ıtty matter behind. The yellow solution is filtered, mixed with a considerable ity of alcohol, and left to stand for a day or two in the dark, when it deposits ıls of chrysophyll mixed with fatty matter. The deposit is filtered off, and d, without removal from the filter, in a hot water funnel; here it is treated a little hot glacial acetic acid. This removes all the fatty, along with some ring, matter. The residue is dissolved in a little chloroform, and the solution, g been mixed with several times its volume of absolute alcohol, is left to stand dark. The next day a quantity of chrysophyll will have separated in crystals golden lustre and of a deep orange or red colour by transmitted light. The ınce is rapidly bleached on exposure to air. In order to preserve it unchanged, uld, after filtration and rapid drying, be put into a glass tube through which ·ent of hydrogen is passed before sealing, then kept in the dark. According naud ('Compt. Rend.' vol. 102, p. 1119, and vol. 104, p. 1293), chrysophyll is cal with carotin. There can be no doubt that it contributes to the obscura- t the blue end of the ordinary chlorophyll spectrum; I have found it accom- ng chlorophyll in all leaves that I have examined. Its solutions when ently dilute show two broad bands at the blue end, without the least trace of ›tion at any other part of the spectrum (fig. 1).

FIG. 1.




*Absorption Spectrum of Chrysophyll.*

sides of the glass. These needles continue to increase in quantity for some time; they are filtered off, washed with alcohol, and then treated with boiling ether, which removes a quantity of fatty matter, at the same time dissolving some of the substance itself. The residue is dissolved in a small quantity of chloroform, and the solution which is deeply coloured is then mixed with several times its volume of absolute alcohol. On standing, the liquid deposits a quantity of long crystalline needles, which are collected on a filter and washed with alcohol, in which they are only slightly soluble. The substance thus obtained is apparently an ethyl compound, and is probably the ethyl ether of the product formed by the action of alkalis on phyllocyanin, this being the conclusion to which its reactions seem to point. In mass it appears of a fine purplish-blue, and shows a semi-metallic lustre. Under the microscope it is seen to consist of acicular crystals, which are mostly opaque, but when very thin are transparent, and appear pale olive-coloured by transmitted light. It softens at 205° C., but it has no definite melting-point. When strongly heated in a glass tube it is decomposed without yielding any crystalline sublimate, leaving a voluminous charcoal; heated on platinum it burns away without leaving any ash. It is insoluble in water, sparingly soluble in boiling alcohol and ether, more easily soluble in benzol and carbon disulphide, and very easily soluble in chloroform. The solutions when diluted have a dull-purplish or pink colour, and show an absorption spectrum identical with one already depicted as belonging to one of the derivatives of phyllocyanin ('Proceedings,' vol. 42, Plate 1, fig. 13). It dissolves in boiling glacial acetic acid and crystallises out on cooling. It is also soluble in concentrated hydrochloric acid, giving a solution which has the same greenish-blue colour, and shows the same absorption-bands as a solution of phyllocyanin in the same menstruum, but on the addition of water it is precipitated unchanged. The quantity of the product, in a crude state, obtained by the method described, amounted to 4·5 parts from 1000 of dry grass.

When methylic alcohol is employed in the extraction of leaves, and the same process as that above described is gone through, a similar compound is obtained, but differing from it in some respects. It crystallises in lustrous purple needles, rather lighter in colour than those from ethylic alcohol; it has no definite melting point; it is hardly soluble in boiling alcohol or ether, but easily soluble in chloroform, the solution showing the same absorption-bands as that of the other compound. It can hardly be doubted that this is the corresponding methyl ether.

These compounds are insoluble in aqueous alkalis, and are very little changed by prolonged boiling therewith, but on treatment with alcoholic potash or soda they are immediately dissolved and decomposed. The process is apparently one of saponification, the produc[t]

being the substance of which the compounds are the ethyl and methyl ethers respectively. In order to obtain this product the ethyl compound is treated with boiling alcoholic soda, in which it readily dissolves. The solution on standing deposits a sodium compound in the shape of a dark-green, almost black, semi-crystalline mass, which is filtered off, washed with absolute alcohol, and dissolved in water. The dark-green solution gives with acetic acid, of which a great excess must be avoided, a green flocculent precipitate, which is filtered off, thoroughly washed with water, and dissolved in ether. On slow evaporation the ethereal solution yields lustrous purple crystals, which must be separated before the solution has quite evaporated, for if there be any free acid present this will after most of the ether has evaporated, begin to act on the substance, inducing a change to which I shall allude presently.

The substance thus prepared is identical with that formed directly by the action of alkali on phyllocyanin; but by the process just described it is obtained in a state of much greater purity than by the direct method. Having read nearly everything that has been written on the chemistry of chlorophyll, I have come to the conclusion that this substance has never previously been described, and I think myself entitled therefore to give it a name. I propose to call it *Phyllotaonin* (from ταών, a peacock).

*Properties of Phyllotaonin.*—On spontaneous evaporation of its ethereal solution, it is obtained in regular flattened crystals or crystalline scales, which by reflected light appear of a fine peacock or steel-blue colour; the crystals are mostly opaque, but when very thin they are transparent and then appear brown by transmitted light. It melts at 184° to a brown resinous mass, but partial decomposition results from fusion, since the melted mass is no longer entirely soluble in chloroform, a little carbonaceous matter being left undissolved. Heated on platinum it swells up, giving off much gas and leaving a voluminous coal which burns away without residue; heated in a tube it swells and is charred without giving any perceptible sublimate. Phyllotaonin is insoluble in boiling water. It is easily soluble in boiling alcohol and ether, but it does not crystallise out on the solutions cooling; the solutions have the same colour, and show exactly the same absorption-bands as solutions of phyllocyanin, but if the least trace of any acid be present in the solution the spectrum gradually changes, the third band from the red end becoming fainter, while the fourth band as well as the first splits up into two. It is soluble in benzol and carbon disulphide, and very easily soluble in chloroform and aniline, but insoluble in ligroin. Phyllotaonin is easily soluble in glacial acetic acid, giving a solution of a fine violet colour, which shows a spectrum differing from that of the ethereal solution, and by this means it may be at once distinguished from

phyllocyanin, which dissolves in ether and in acetic acid, both solutions having a dull green colour, and showing the same spectrum. It is also soluble in concentrated hydrochloric acid, the solution having a bright bluish-green colour.

In contact with acids phyllotaonin undergoes a series of changes, accompanied by corresponding changes in the absorption spectrum. If to an ethereal or alcoholic solution of phyllotaonin a small quantity of an acid, such as hydrochloric, sulphuric, oxalic, tartaric or acetic acid be added, the colour of the solution changes slowly from green to brown, and now shows the spectrum frequently referred to in which two bands, that in the red and that in the green, are seen split up into two (see fig. 11 of the Plate previously referred to). A further change takes place on standing, one of the bands in the green becoming darker, the other lighter (see spectrum, fig. 12). Here the action stops with all the acids named except acetic acid. On treating phyllotaonin with boiling glacial acetic acid it dissolves, and the dark purple solution if sufficiently concentrated deposits on cooling crystalline needles, arranged in fan-shaped masses. These collected on a filter and dried show a fine purple colour, and closely resemble the supposed ethyl-compound of phyllotaonin; its solutions show the same absorption spectrum as the latter. This product is doubtless a compound with acetic acid; stronger acids such as sulphuric or hydrochloric acid yield no similar compounds. The products formed by the action of acids may in all cases be re-converted into phyllotaonin by means of alkali. The process of re-conversion may be traced in its course with the crystallised acetate. If the latter be treated with aqueous potash in the cold it dissolves; acetic acid added gives a green precipitate which dissolves in ether, the solution showing the spectrum of fig. 11, but if boiling alcoholic potash be employed, the corresponding ethereal solution shows the spectrum of phyllotaonin. Under the influence of acetic acid the latter again passes through the series of changes previously described. That the changes induced on the one hand by acids, and on the other by alkalis, are due in one case to hydration and in the other to dehydration, seems probable. After being heated to the melting point, phyllotaonin gives solutions showing the spectrum, fig. 12, but by treatment of the fused substanee with alcoholic potash it returns to its original state. It is difficult to attribute the change in this case to anything but loss of water, the latter being taken up again on treatment with alkali.

A potassium compound of phyllotaonin is obtained on adding potash to an alcoholic solution of the substance; it crystallises in needles which are purple by reflected light. The sodium compound obtained in the same way is hardly crystalline. A boiling alcoholic solution of phyllotaonin to which cupric acetate and acetic acid hav

been added, deposits on cooling and standing a quantity of crystalline needles arranged in pretty rosettes which, after filtering off and drying, appear bluish-green by reflected as well as by transmitted light, and show no metallic lustre; the alcoholic solution of this compound shows the same absorption spectrum as that of the corresponding phyllocyanin compound, and, like the latter, it is not decomposed nor in any way changed by treatment with boiling hydrochloric acid. Similar compounds containing iron and silver may be obtained, but their properties are not sufficiently interesting to merit detailed description; they resemble the corresponding phyllocyanin compounds.

On adding metallic tin to a solution of phyllotaonin in hydrochloric acid and allowing to stand, the solution soon loses its bright bluish-green colour, and becomes olive-green, finally reddish-yellow. Water now gives a red precipitate, which filtered off and washed dissolves in alcohol with a crimson colour, the solution showing a spectrum similar to that of the final product of the action of tin and hydrochloric acid on phyllocyanin.

Though there can be little doubt as to the purple crystals formed by the action of hydrochloric acid on an alcoholic solution of alkaline chlorophyll being an ether, I have not succeeded in reproducing it by the direct action of acid on an alcoholic solution of phyllotaonin. The solution retains its bluish-green colour unchanged, deposits no crystals even on long standing, and gives with water a precipitate consisting of uncombined phyllotaonin. A compound resembling that in the purple crystals may, however, be formed from phyllotaonin by a different process. If to an alcoholic solution of phyllotaonin ethyl iodide and a little caustic potash be added, the solution on boiling deposits a small quantity of a black powder, which being collected on a filter and treated with dilute acid, is found to be soluble in alcohol, ether, and chloroform, giving purple solutions which show the same spectrum as solutions of the purple crystals. It is, however, easily soluble in aqueous alkali, and may therefore be a mono-ethyl, the other being a di-ethyl ether. It is probably identical with the compound formed directly from phyllocyanin by a similar process, as described in the first part of this memoir, in the solutions of which the spectrum (fig. 13) so frequently referred to was first observed. This very peculiar spectrum belongs, it appears, to four distinct compounds.

In order to explain the formation of the purple crystals by the process above described, we may suppose that by the influence of alkalis chlorophyll is first converted into a substance which by decomposition with acids yields phyllotaonin, and this in the nascent state and in contact with alcohol and hydrochloric acid undergoes etherification.

*Of the compounds* above described I have analysed such as

well crystallised and appeared to be pure, but I will not give the results until I have had an opportunity of confirming them with freshly prepared material. The difficulty experienced in preparing sufficient quantities of pure substances from chlorophyll has proved a great drawback in this investigation and has much retarded its progress.

My friend Dr. Burghardt, of the Owens College, has had the kindness to examine at my request the crystalline form (fig. 2) of phyllotaonin, and reports as follows :—

FIG. 2.

Crystal system monosymmetrical, oblique rectangular prism, formed by the combination of the ortho- and clino-pinacoids.

The terminal faces are a negative hemipyramid. The faces *b* (010 Miller or $\infty \breve{P} \infty$ Naumann) predominate, giving a "vertical tabular habit" to the crystal.

It was impossible to obtain any measurements of the angles owing to the smallness of the crystals and the roughness of the faces. The value, therefore, of the hemipyramid indices is unknown. The faces "*a*" are 100 (Miller) or $\infty \bar{P} \infty$ (Naumann), whilst the faces "*c*" are the negative hemipyramid $-hkl$ (Miller) or $-mP$ (Naumann).

They cleave parallel to the ortho-pinacoid distinctly. Examined in polarised light they exhibit depolarisation, on rotating the Nicol's prism the colour changing from a light-yellow to a rich brownish-red.

'A new Method of determining the Number of Micro-organisms in Air." By THOMAS CARNELLEY, D.Sc., Professor of Chemistry, and THOS. WILSON, University College, Dundee. Communicated by Sir HENRY ROSCOE, F.R.S. Received February 3—Read February 16, 1888.

The subject of bacteriology has of late excited considerable interest, ıd is at present studied by a great number of investigators, both in ıis country and on the Continent. Under these circumstances a new ıd improved method for the bacterioscopic analysis of air will be of ıterest.

There are several methods at present in use for this purpose, but will only be necessary to refer to two of these, in both of which ›lid media are employed.

1. *Hesse's Method* ('Mittheilungen aus dem Kaiserlichen Gesund-eitsamte,' vol. 2, p. 182).—This is the oldest process in which a ›lid medium is used for the nutrition of the micro-organisms, and is ıe one which has been most commonly employed. The principle of ıe process consists in drawing a known volume of air through a long ide tube, the inside of which is coated with Koch's nutrient gela-ne-peptone. As the air passes through the tube the micro-organisms ›ttle on the jelly, and in the course of a few days develop into ›oglea or colonies, and thus become visible to the naked eye and may e counted.

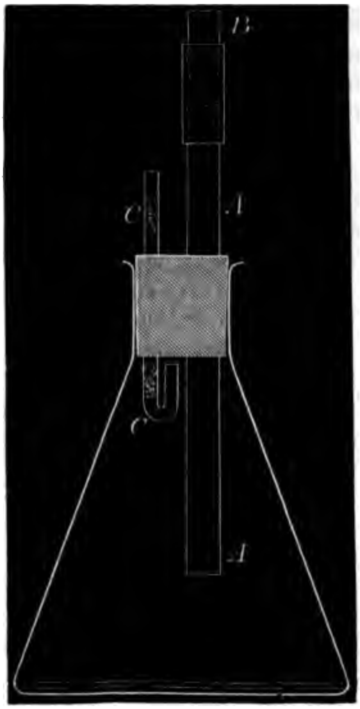
2. *Dr. Percy Frankland's Method* ('Roy. Soc. Proc.,' vol. 41, p. 443; Phil. Trans.,' B, vol. 178 (1887), p. 113).—This method consists ısentially in aspirating a known volume of air through a small glass ıbe containing two sterile plugs consisting either of glass-wool alone r of glass-wool coated with sugar. After a given volume of air has een aspirated the two plugs are transferred respectively to two flasks ıch containing melted sterile gelatine-peptone and plugged with ›erile cotton-wool stoppers. The plug is carefully agitated with the ›lly so as to avoid any formation of froth, and when the plug has been ›mpletely disintegrated and mixed with the gelatine the latter is ›ngealed so as to form an even film over the inner surface of the ısk. On incubating these flasks at a temperature of 20° C., the ›lonies soon begin to appear and may be counted.

*New Method.*—The new process which forms the subject of the resent communication is a modification of Hesse's method, in which flask is substituted for a tube.

The flask employed is conical in form and has a capacity of about alf a *litre. The flask* is fitted with a two-holed india-rubber stopper. *hrough one hole* passes the "entrance tube" AA. This is a piece

of glass tube about 8 inches long and $\frac{3}{8}$ inch* internal diameter. It extends about two-thirds of the way down the flask, and is closed at the outer end by a glass stopper B, fitted on with a piece of india-rubber tubing. Into the other hole of the stopper is fitted the "exit tube" CC. This is simply a piece of ordinary glass tubing (about $\frac{1}{4}$ inch

FIG. 1.



diameter) bent round at the lower end so that it opens in the neck of the flask just under the india-rubber stopper. It is open at both ends, but contains two cotton-wool plugs to prevent any micro-organisms passing back into the flask from the outside air.

10 c.c. of Koch's gelatine-peptone are introduced into the flask and the stopper tied on with copper wire. The flask is then sterilised by heating in steam at 100° C. for an hour and allowed to cool, whereby

* The entrance tube must have at least this width, for if it be too narrow, moisture from the jelly forms during sterilisation on the inside of the tube, and on cooling runs down and collects as a drop on the end, so that the air, on entering the flask, has to pass through this drop of water, which thus retains some of the micro-organisms, and so vitiates the results. This, however, is entirely obviated by using a tube of the prescribed width.

an even layer of gelatine solidifies at the bottom of the flask. On taking the flask out of the steriliser it is generally necessary to carefully rinse the jelly round the sides of the flask so as to take up any steam which may have condensed there and which might subsequently collect in drops and run down on to the colonies and inoculate the rest of the jelly.

In doing this care should be taken to avoid frothing of the jelly.

In taking a sample of air the aspirator is attached to the exit-tube C, and the india-rubber tube and stopper B removed from the end of A. A known volume of air is then drawn through the flask, after which the stopper is replaced. As the air passes through the flask the micro-organisms settle on the jelly, and in the course of a few days develop into colonies and may be counted. If there are a large number of micro-organisms present the bottom of the flask may, for convenience in counting, be marked out into squares with ink. The rate of aspiration we have employed is the same as in Hesse's process, viz., about 1 litre in three minutes. Usually the micro-organisms are deposited more or less directly under the lower end of the entrance tube, while none are deposited on the sides of the flask, even though the latter be coated with jelly, which would seem to indicate that no micro-organisms pass over into the exit tube.

At first sight it seemed very likely that on account of the air having to pass through an entrance tube 8 inches long, a number of the micro-organisms might adhere to the side of the tube and never reach the jelly, so that the results obtained would be too low. In order to ascertain whether this was the case or not, a number of flasks were prepared in which the inside of the entrance tube was coated with a thin layer of jelly. The samples of air were then taken in the usual way, and after sufficient time had been allowed for the development of the colonies, the number in the flask and in the entrance tube were counted, with the following results:—

Table I.

| No. | Circumstances. | Vol. of air taken. | No. of colonies in flask. | No. of colonies in entrance tube. |
|---|---|---|---|---|
| 1 | Dusty air .... .. | 400 c.c. | 287 | 3 |
| 2 | Dusty air ..... | 500 " | 145 | 1 |
| 3 | Dusty air ...... | 500 " | At least 100 | 4 |

Unfortunately we omitted to count the colonies in No. 3 for a day or two, when it was found that a number of them had run together, but there were at least 100, and probably many more. The above *results show that only* about 1 per cent. of the micro-organisms

Table II.—Results obtained by Comparative Experiments with Flasks and Hesse Tubes.

| No. | Place. | Date. | Vol. of air taken. | No. of bacteria. | | No. of moulds. | | Total micro-organisms. | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | In Hesse tube. | In flask. | In Hesse tube. | In flask. | In Hesse tube. | In flask. |
| | | 1887. | Litres. | | | | | | |
| 1 | Private laboratory | April 21 | 10 | 11 | 11 | 1 | 0 | 12 | 11 |
| 2 | Outside air (Dundee) | ,, 22 | 10 | 2 | 2 | 1 | 1 | 3 | 3 |
| 3 | Outside, beside macerating tubs† | ,, 23 | 10 | 7 | 6 | 6 | 6 | 13 | 12 |
| 4 | Combustion room (dusty air)‡ | ,, 26 | ½ | 54 | 54 | 3 | 4 | 57 | 58 |
| 5 | Long Wynd School, Room 1 | ,, 27 | ½ | 70 | 61 | 1 | 0 | 71 | 61 |
| 6* | Long Wynd School, Room 2 | ,, 27 | ½ | 43 | 22 | 1 | 1 | 44 | 23 |
| 7* | Outside air (Dundee) | ,, 28 | 10 | 45 | 14 | 2 | 0 | 47 | 14 |
| 8 | Vestry of Church§ | ,, 30 | 5 | 11 | 10 | 97 | 86 | 108 | 96 |
| 9* | Outside air (Dundee) | May 2 | 10 | 54 | 26 | 5 | 0 | 59 | 26 |
| 10 | Combustion room (dusty air)‡ | ,, 2 | ½ | 24 | 19 | 0 | 1 | 24 | 20 |
| 11 | Brown Street School, Room 1 | ,, 11 | ½ | 40 | 40 | 1 | 1 | 41 | 41 |
| 12 | Brown Street School, Room 2 | ,, 11 | ½ | 16 | 15 | 0 | 1 | 16 | 16 |
| 13 | Hunter Street School, Room 1 | ,, 12 | ½ | 79 | 95 | 0 | 0 | 79 | 95 |
| 14 | Hunter Street School, Room 2 | ,, 12 | ½ | 94 | 94 | 1 | 0 | 95 | 94 |
| 15* | Balfour Street School | ,, 17 | ½ | 34 | 3 | 3 | 0 | 37 | 3 |
| 16* | Outside, beside macerating tubs† | ,, 19 | 5 | 55 | 11 | 5 | 0 | 60 | 11 |
| 17 | Long Wynd School, Room 1 | ,, 20 | ½ | 19 | 16 | 0 | 0 | 19 | 16 |
| 18 | Long Wynd School, Room 2 | ,, 20 | ½ | 26 | 26 | 2 | 0 | 28 | 26 |
| 19* | Outside air (Dundee) | ,, 21 | 5 | 20 | 5 | 2 | 0 | 22 | 5 |
| 20 | Combustion room (dusty air)‡ | ,, 23 | ½ | 115 | 112 | 16 | 14 | 131 | 126 |
| 21 | Combustion room (dusty air)‡ | ,, 23 | ½ | 71 | 78 | 11 | 10 | 82 | 88 |

* Results non-concordant.

† These experiments were made outside, close to tubs in which a number of animals were macerating for the Biological Museum.

‡ Dusty air produced by shaking door mats.

§ This room is used as a dissecting-room in connexion with the Biological Department. The large number of moulds found in this sample is noteworthy.

adhered to the sides of the entrance tube, even when the latter was coated with jelly, so that under ordinary conditions the number so adhering would probably be very much less. This apparent source of error, therefore, may be entirely neglected when the width of the entrance tube is not less than that prescribed.

In order to test the quantitative accuracy of the method, a number of comparative experiments were made by collecting samples of air simultaneously in the flasks and in Hesse tubes, placed side by side. On p. 458 is a table of the results obtained in this way. In comparing these results it must not be forgotten that, even when two Hesse tubes are compared the one against the other, it is only occasionally that identical numbers are obtained in each tube. Thus one may get six in one tube and eight in the other, or twenty in one tube and twenty-three in the other, and so on, the difference varying according to the total number of micro-organisms present.

From the above table it will be seen that in nearly all cases the number of micro-organisms (both bacteria and moulds) in the tube and in the flask correspond almost exactly. In Nos. 6, 7, 9, 15, 16, and 19, however, this is very far from being the case, for in each of these the flask method gave very much lower results than the Hesse tube. Of these six non-concordant experiments, four were made in outside air, and the other two in schoolrooms in which there was a considerable draught, for the day being warm, the windows and doors were all open.

Now Dr. Percy Frankland (*loc. cit.*) has conclusively proved that Hesse's method does not give reliable results for outside air, except on calm days. He made a number of experiments in which a control tube was used side by side with the aspirated tube, and in this way he was able to obtain a rough idea of the number of micro-organisms which gain access to a Hesse tube, irrespective of aspiration. In illustration of this we may quote a few of his results:—

Table III.

| No. | State of wind. | Vol. of air taken. | Micro-organisms in aspirated tube. | Micro-organisms in non-aspirated tube. |
|---|---|---|---|---|
| 1 | Moderate........... | 12 litres. | 158 | 54 |
| 2 | Slight.............. | 12 „ | 12 | 3 |
| 3 | Moderately strong... | 12 „ | 53 | 11 |
| 4 | Moderately strong... | 12 „ | 114 | 34 |
| 5 | Moderate, but variable | 12 „ | 49 | 29 |
| 6 | Moderate .......... | 11 „ | 52 | 15 |
| 7 | Strong............ | 10 „ | 75 | 15 |
| 8 | Strong............. | 12 „ | 78 | 48 |
| 9 | *Slight*............. | 12 „ | 72 | 27 |

From these experiments it is evident that Hesse's method is not reliable for outside air, except when there is little or no wind.

By reference to Table II it will be observed that, of the six experiments made in outside air only two were concordant, the discrepancy in the other four being very considerable. In order to learn if this discrepancy was due to the effect of the wind, the state of the latter was ascertained from the Observatory at the Dundee Harbour, for all the dates on which experiments had been made in outside air. The results were as follows:—

Table IV.

| No. | Direction of wind. | Miles per hour. | Wind as felt. | Date. | Micro-organisms in Hesse tube. | Micro-organisms in flask. |
|---|---|---|---|---|---|---|
| 2 | S.W. | 7 | Little or none. | April 22nd. | 3 | 3 |
| 3 | S. | 5½ | Little or none. | April 23rd. | 13 | 12 |
| 7 | S.W. to S. | 6 | Might be gusty. | April 28th. | 47 | 14 |
| 9 | E. to N.E. | 11 | Steady. | May 2nd. | 59 | 26 |
| 16 | W. to S.W. | 13½ | Gusty. | May 19th. | 60 | 11 |
| 19 | W. to N.W. | 9½ | Gusty. | May 21st. | 22 | 5 |

In the two cases in which the number of micro-organisms in the flask corresponded with that in the tube, little or no wind was felt, and the wind was travelling at the rate of about 6 miles per hour; whereas in the other four cases in which discordant results were obtained, the wind was travelling at an average of about 10 miles per hour, and was gusty besides. It would seem, therefore, that the flask method gives more correct results than Hesse tubes for outside air when there is any aerial disturbance.

The only two cases in which there was any discrepancy for inside air were Nos. 6 and 15. Both of these were samples of school air, and it was noted at the time the samples were taken that in both cases there was a considerable draught through the rooms, for the day being warm, the windows and doors were all open. On comparing the determinations of carbonic acid made in these rooms at the same time, it was found that in both they were *comparatively* very low, viz., 10·6 vols. per 10,000 in No. 6, and 7·3 vols. in No. 15; whereas average school air in Dundee contains about 19 vols. of carbonic acid per 10,000. This comparatively low amount of carbonic acid can only be accounted for by the fact that there must have been a draught in the room at the time the experiments were made.

Experiments were also made in order to ascertain if any micro-

organisms gained entrance to the flasks irrespective of aspiration, corresponding experiments being made simultaneously with Hesse tubes. For this purpose a pair of flasks and a pair of Hesse tubes were simultaneously exposed to the outside air for the same length of time, but without aspirating air through any of them. The exit tube (which in an ordinary experiment is connected with the aspirator) of one of each pair of flasks and tubes was stoppered, and the exit tube of the other flask and tube left unstoppered. The entrance to each flask and tube was of course left open. The total number of colonies obtained in each case were as follows, the numbers in brackets being the number of moulds:—

Table V.

| No. | State of wind. | Time of exposure. | Hesse tubes. | | Flasks. | |
|---|---|---|---|---|---|---|
| | | | Stoppered. | Unstoppered. | Stoppered. | Unstoppered. |
| 1 | Very strong...... | ½ hour | .. | 23 [1] | .. | 2 [1] |
| 2 | Gentle .......... | 1 „ | 2 [2] | 1 [1] | 0 | 0 |
| 3 | Gentle .... .. . | 1 „ | 6 [5] | 1 [1] | 0 | 0 |
| 4 | Moderately strong and variable.... | ½ „ | 8 [6] | 12 [5] | .. | 0 |
| 5 | Rather strong and variable ....... | ½ „ | 8 [0] | 12 [0] | 0 | 1 [0] |
| 6 | Rather strong and variable ....... | ½ „ | 45 [2] | 33 [1] | 0 | 1 [0] |

Thus out of ten flasks exposed to the air for half to one hour, only three were contaminated, and these only very slightly, and on very windy days, whereas the Hesse tubes were considerably contaminated. It is thus seen that the flask method, unlike the Hesse tube method, is practically free from *vitiation* by aerial disturbance.

We can fully confirm Dr. P. Frankland's statement that Hesse's method gives good results in cases where the air is still and free from draughts, as in most inside buildings and outside on calm still days, for under these conditions Hesse's method agrees remarkably well both with Frankland's process and with our own; whereas in a disturbed atmosphere, as in outside air on windy days, or in buildings where a strong draught prevails, Hesse's method gives results which are considerably in excess of those obtained either by Frankland's method or by our own.

The following are the chief advantages of the new method:—

(1.) It possesses, in common with Hesse's and Frankland's processes, *the advantages of a solid* nutrient medium.

(2.) It gives accurate results, as shown by comparative tests.

(3.) There is no risk of aerial contamination either during the preparation of the flasks previous to use, or subsequently during the growth of the colonies.

(4.) It is very much cheaper than Hesse's method, for a flask fitted ready for use costs only about 1*s*. 3*d*. (exclusive of jelly), where a Hesse's tube costs about 3*s*. This is a very material item when a large number of experiments are to be made.

(5.) The flasks being of thin glass very rarely break during sterilisation, whereas this is a serious source of annoyance and expense in the case of Hesse's tubes.

(6.) There is not the least chance of leakage during sterilisation, as sometimes occurs with Hesse's tubes, for in the latter method the india-rubber caps have to be very carefully fitted on, since with the slightest crease in the india-rubber the tubes are sure to leak during sterilisation, with consequent loss of jelly, which entails refitting and refilling.

(7.) There is a great saving in jelly. A flask needs only 10 c.c., or one-fifth the quantity required by a Hesse tube. In a long series of experiments the cost of jelly is very considerable, both in the expense of the materials and the time required to make it.

(8.) In common with Frankland's process the flask method is free from errors arising from "aerial currents," which are sometimes so serious a source of error in Hesse's tubes when employed for determinations in outside air, such currents being apt to blow micro-organisms into a Hesse tube over and above those contained in the volume of air aspirated.

(9.) An advantage which the flask method possesses over Frankland's process is that in the former the micro-organisms pass directly on to the nutrient jelly in the flask, whereas in the latter they are first entangled in the glass-wool filter, and afterwards transferred to the cultivating medium, when they are disentangled from the glass-wool by agitation with the jelly, an operation which would seem to require considerable care. Again, in Frankland's process the micro-organisms are embedded in the mass of the jelly, while in our method they fall and grow directly on the surface.

(10.) On the other hand Frankland's method possesses two important advantages; first, on account of the small size of his filter tubes, they admit of being carried from place to place without inconvenience, whereas flasks and Hesse tubes are comparatively bulky. This is a great point when a large number of determinations are to be made at different places away from the laboratory. Second, the air can be aspirated through one of Frankland's filters about four times as fast as through a Hesse's tube, which is of considerable advantage in the case of determinations in outside air, where at least 10 litres require

to be aspirated, though it is of no consequence for the air of buildings where the aspiration of only one-half, or at most 1 litre of air is necessary, and occupies less than two minutes. The rate of aspiration we have employed with our own method has been the same as with Hesse tubes, viz., 1 litre in three minutes. It is not at all unlikely, however, that a more rapid rate might be adopted without affecting the accuracy of the results.

## Addendum. Received April 22, 1888.

The following experiments were made for the purpose of testing whether any micro-organisms pass into the exit tube or become attached to the under side of the cork.

### A. *As regards the Passage of Organisms into the Exit Tube.*

In these experiments, the flask was fitted up and charged with jelly in the ordinary manner, except that a little jelly was also placed in the bend of the exit tube. The whole was then sterilised as usual, and, during the subsequent cooling, the flask was so manipulated that a coating of jelly was formed over the inside walls of the exit tube, keeping clear, however, of the cotton-wool plugs. Half a litre of air was then drawn through each flask at the rate of 1 litre in three minutes. The samples were collected in a room in which a slight dust had been raised by the shaking of a door-mat. After the lapse of eight days, the number of colonies counted in each flask was as follows. In no case were any colonies found in the exit tube.

| | Per ½ litre of air. | | |
|---|---|---|---|
| | In flask. | In exit tube. | |
| Experiment I .. | About 300 | 0 | Collected just after raising of dust. |
| Experiment II .. | About 200 | 0 | Collected after a few minutes' interval. |
| Experiment III .. | About 250 | 0 | Collected after a few minutes' interval. |
| Experiment IV .. | About 180 | 0 | Collected after a further interval of a few minutes. |

### B. *As regards the Attachment of Organisms to the Under Side of the Cork.*

The flasks were charged and sterilised in the ordinary way, but *during cooling, after* sterilisation, the flask was so manipulated as to

allow the jelly to form a thin coating over the under side of the cork. Half a litre of air was then drawn through each flask at the rate of 1 litre in three minutes. The samples were collected as before, except that the dust raised was not nearly so great. After nine days, the following number of colonies had developed on the jelly in the flasks, but not a single one was observed on the under side of the cork:—

| | Per ½ litre of air. | | |
|---|---|---|---|
| | In flask. | On cork. | |
| Experiment I .. | 57 | 0 | Collected just after raising of dust. |
| Experiment II .. | 23 | 0 | Collected after an interval of a few minutes. |

The above results show, therefore, that, with an aspiration of 1 litre of air in three minutes, *all* the organisms are deposited on the jelly at the bottom of the flask, and that none reach the cork or exit tube. This result is probably due not only to the action of gravity, but also to the initial velocity, with which the organisms leave the mouth of the entrance tube and enter the flask, being such as to project them on to the surface of the jelly at the bottom of the flask, where they stick and have not the chance of rising again.

## OBITUARY NOTICES OF FELLOWS DECEASED.

Charles Robert Darwin was the fifth child and second son of Robert Waring Darwin and Susannah Wedgwood, and was born on the 12th February, 1809, at Shrewsbury, where his father was a physician in large practice.

Mrs. Robert Darwin died when her son Charles was only eight years old, and he hardly remembered her. A daughter of the famous Josiah Wedgwood, who created a new branch of the potter's art, and established the great works of Etruria, could hardly fail to transmit important mental and moral qualities to her children; and there is a solitary record of her direct influence in the story told by a school-fellow, who remembers Charles Darwin "bringing a flower to school, and saying that his mother had taught him how, by looking at the inside of the blossom, the name of the plant could be discovered." (I, p. 28.*)

The theory that men of genius derive their qualities from their mothers, however, can hardly derive support from Charles Darwin's case, in the face of the patent influence of his paternal forefathers. Dr. Darwin, indeed, though a man of marked individuality of character, a quick and acute observer, with much practical sagacity, is said not to have had a scientific mind. But when his son adds that his father "formed a theory for almost everything that occurred" (I, p. 20), he indicates a highly probable source for that inability to refrain from forming an hypothesis on every subject which he confesses to be one of the leading characteristics of his own mind, some pages further on (I, p. 103). Dr. R. W. Darwin, again, was the third son of Erasmus Darwin, also a physician of great repute, who shared the intimacy of Watt and Priestley, and was widely known as the author of 'Zoonomia,' and other voluminous poetical and prose works which had a great vogue in the latter half of the eighteenth century. The celebrity which they enjoyed was in part due to the attractive style (at least according to the taste of that day) in which the author's extensive, though not very profound, acquaintance with natural phenomena was set forth; but in a still greater degree, probably, to the boldness of the speculative views, always ingenious and sometimes fantastic, in which he indulged. The conception of evolution set afoot by De Maillet and others, in the early part of the century, not only found a vigorous champion in

* The references throughout this notice are to the 'Life and Letters,' unless the *contrary is expressly stated.*

Erasmus Darwin; but he propounded an hypothesis as to the manner in which the species of animals and plants have acquired their characters, which is identical in principle with that subsequently rendered famous by Lamarck.

That Charles Darwin's chief intellectual inheritance came to him from the paternal side then, is hardly doubtful. But there is nothing to show that he was, to any sensible extent, directly influenced by his grandfather's biological work. He tells us that a perusal of the 'Zoonomia' in early life produced no effect upon him, although he greatly admired it—and that on reading it again, ten or fifteen years afterwards, he was much disappointed, "the proportion of speculation being so large to the facts given." But with his usual anxious candour he adds, "Nevertheless, it is probable that the hearing, rather early in life, such views maintained and praised, may have favoured my upholding them, in a different form, in my 'Origin of Species.'" (I, p. 38.) Erasmus Darwin was in fact an anticipator of Lamarck, and not of Charles Darwin; there is no trace in his works of the conceptions by the addition of which his grandson metamorphosed the theory of evolution as applied to living things and gave it a new foundation.

Charles Darwin's childhood and youth afforded no intimation that he would be, or do, anything out of the common run. In fact, the prognostications of the educational authorities into whose hands he first fell, were most distinctly unfavourable; and they counted the only boy of original genius who is known to have come under their hands as no better than a dunce. The history of the educational experiments to which Darwin was subjected is curious, and not without a moral for the present generation. There were four of them, and three were failures. Yet it cannot be said that the materials on which the pedagogic powers operated were other than good. In his boyhood, Darwin was strong, well-grown, and active, taking the keen delight in field sports and in every description of hard physical exercise which is natural to an English country-bred lad; and, in respect of things of the mind, he was neither apathetic, nor idle, nor one-sided. The 'Autobiography' tells us that he "had much zeal for whatever interested" him, and he was interested in many and very diverse topics. He could work hard, and liked a complex subject better than an easy one. The "clear geometrical proofs" of Euclid delighted him. His interest in practical chemistry, carried out in an extemporised laboratory, in which he was permitted to assist by his elder brother, kept him late at work, and earned him the nickname of "gas" among his schoolfellows. And there could have been no insensibility to literature in one who, as a boy, could sit for hours reading Shakespeare, Milton, Scott, and Byron; who greatly admired some of the Odes of Horace; and who, in later years, on board the

"Beagle," when only one book could be carried on an expedition, chose a volume of Milton for his companion.

Industry, intellectual interests, the capacity for taking pleasure in deductive reasoning, in observation, in experiment, no less than in the highest works of imagination: where these qualities are present any rational system of education should surely be able to make something of them. Unfortunately for Darwin, the Shrewsbury Grammar School, though good of its kind, was an institution of a type universally prevalent in this country half a century ago, and by no means extinct at the present day. The education given was "strictly classical," "especial attention" being "paid to verse-making," while all other subjects, except a little ancient geography and history, were ignored. Whether, as in some famous English schools at that date and much later, elementary arithmetic was also left out of sight does not appear; but the instruction in Euclid which gave Charles Darwin so much satisfaction was certainly supplied by a private tutor. That a boy, even in his leisure hours, should permit himself to be interested in any but book-learning seems to have been regarded as little better than an outrage by the head master, who thought it his duty to administer a public rebuke to young Darwin for wasting his time on such a contemptible subject as chemistry. English composition and literature, modern languages, modern history, modern geography, appear to have been considered to be as despicable as chemistry.

For seven long years, Darwin got through his appointed tasks; construed without cribs, learned by rote whatever was demanded, and concocted his verses in approved schoolboy fashion. And the result, as it appeared to his mature judgment, was simply negative. "The school as a means of education to me was simply a blank." (I, p. 32.) On the other hand, the extraneous chemical exercises, which the head master treated so contumeliously, are gratefully spoken of as the "best part" of his education while at school. Such is the judgment of the scholar on the school; as might be expected, it has its counterpart in the judgment of the school on the scholar. The collective intelligence of the staff of Shrewsbury School could find nothing but dull mediocrity in Charles Darwin. The mind that found satisfaction in knowledge, but very little in mere learning, that could appreciate literature, but had no particular aptitude for grammatical exercises, appeared to the "strictly classical" pedagogue to be no mind at all. As a matter of fact, Darwin's school education left him ignorant of almost all the things which it would have been well for him to know, and untrained in all the things it would have been useful for him to be able to do, in after life. Drawing, practice in English composition, and instruction in the elements of the physical sciences, would not only have been infinitely valuable *to him in reference to his* future career, but would have furnished

the discipline suited to his faculties, whatever that career might be. And a knowledge of French and German, especially the latter, would have removed from his path obstacles which he never fully overcame.

Thus, starved and stunted on the intellectual side, it is not surprising that Charles Darwin's energies were directed towards athletic amusements and sport, to such an extent, that even his kind and sagacious father could be exasperated into telling him that "he cared for nothing but shooting, dogs, and rat-catching." (I, p. 32.) It would be unfair to expect even the wisest of fathers to have foreseen that the shooting and the rat-catching, as training in the ways of quick observation and in physical endurance, would prove more valuable than the construing and verse-making to his son, whose attempt, at a later period of his life, to persuade himself "that shooting was almost an intellectual employment: it required so much skill to judge where to find most game, and to hunt the dogs well" (I, p. 43), was by no means so sophistical as he seems to have been ready to admit.

In 1825, Dr. Darwin came to the very just conclusion that his son Charles would do no good by remaining at Shrewsbury School, and sent him to join his elder brother Erasmus, who was studying medicine at Edinburgh, with the intention that the younger son should also become a medical practitioner. Both sons, however, were well aware that their inheritance would relieve them from the urgency of the struggle for existence which most professional men have to face, and they seem to have allowed their tastes, rather than the medical curriculum, to have guided their studies. Erasmus Darwin was debarred by constant ill-health from seeking the public distinction which his high intelligence and extensive knowledge would, under ordinary circumstances, have insured. He took no great interest in biological subjects, but his companionship must have had its influence on his brother. Still more was exerted by friends like Coldstream and Grant, both subsequently well-known zoologists (and the latter an enthusiastic Lamarckian), by whom Darwin was induced to interest himself in marine zoology. A notice of the ciliated germs of *Flustra*, communicated to the Plinian Society in 1826, was the first fruits of Darwin's half century of scientic work. Occasional attendance at the Wernerian Society brought him into relation with that excellent ornithologist the elder Macgillivray, and enabled him to see and hear Audubon. Moreover, he got lessons in bird-stuffing from a negro, who had accompanied the eccentric traveller Waterton in his wanderings, before settling in Edinburgh.

No doubt Darwin picked up a great deal of valuable knowledge during his two years' residence in Scotland; but it is equally clear that next to none of it came through the regular channels of academic education. Indeed, the influence of the Edinburgh professoriate

appears to have been mainly negative, and in some cases deterrent; creating in his mind, not only a very low estimate of the value of lectures, but an antipathy to the subjects which had been the occasion of the boredom inflicted upon him by their instrumentality. With the exception of Hope, the Professor of Chemistry, Darwin found them all "intolerably dull." Forty years afterwards he writes of the lectures of the Professor of Materia Medica that they were "fearful to remember." The Professor of Anatomy made his lectures "as dull as he was himself," and he must have been very dull to have wrung from his victim the sharpest personal remark recorded as his. But the climax seems to have been attained by the Professor of Geology and Zoology, whose prælections were so "incredibly dull" that they produced in their hearer the somewhat rash determination never "to read a book on geology or in any way to study the science" so long as he lived. (I, p. 41.)

There is much reason to believe that the lectures in question were eminently qualified to produce the impression which they made; and there can be little doubt, that Darwin's conclusion that his time was better employed in reading than in listening to such lectures was a sound one. But it was particularly unfortunate that the personal and professorial dulness of the Professor of Anatomy, combined with Darwin's sensitiveness to the disagreeable concomitants of anatomical work, drove him away from the dissecting room. In after life, he justly recognised that this was an "irremediable evil" in reference to the pursuits he eventually adopted; indeed, it is marvellous that he succeeded in making up for his lack of anatomical discipline, so far as his work on the Cirripedes shows he did. And the neglect of anatomy had the further unfortunate result that it excluded him from the best opportunity of bringing himself into direct contact with the facts of nature which the University had to offer. In those days, almost the only practical scientific work accessible to students was anatomical, and the only laboratory at their disposal the dissecting room.

We may now console ourselves with the reflection that the partial evil was the general good. Darwin had already shown an aptitude for practical medicine (I, p. 37); and his subsequent career proved that he had the making of an excellent anatomist. Thus, though his horror of operations would probably have shut him off from surgery, there was nothing to prevent him (any more than the same peculiarity prevented his father) from passing successfully through the medical curriculum and becoming, like his father and grandfather, a successful physician, in which case 'The Origin of Species' would not have been written. Darwin has jestingly alluded to the fact that the shape of his nose (to which Captain Fitzroy objected), nearly prevented his embarkation in the "Beagle'; it may be that the sensitiveness of that organ secured him for science.

At the end of two years' residence in Edinburgh, it hardly needed Dr. Darwin's sagacity to conclude that a young man, who found nothing but dulness in professorial lucubrations, could not bring himself to endure a dissecting room, fled from operations, and did not need a profession as a means of livelihood, was hardly likely to distinguish himself as a student of medicine. He therefore made a new suggestion, proposing that his son should enter an English University and qualify for the ministry of the Church. Charles Darwin found the proposal agreeable, none the less, probably, that a good deal of natural history and a little shooting were by no means held, at that time, to be incompatible with the conscientious performance of the duties of a country clergyman. But it is characteristic of the man, that he asked time for consideration, in order that he might satisfy himself that he could sign the Thirty-nine Articles with a clear conscience. However, the study of "Pearson on the Creeds" and a few other books of divinity soon assured him that his religious opinions left nothing to be desired on the score of orthodoxy, and he acceded to his father's proposition.

The English University selected was Cambridge; but an unexpected obstacle arose from the fact that, within the two years which had elapsed since the young man who had enjoyed seven years of the benefit of a strictly classical education had left school, he had forgotten almost everything he had learned there, "even to some few of the Greek letters." (I, p. 46.) Three months with a tutor, however, brought him back to the point of translating Homer and the Greek Testament "with moderate facility," and Charles Darwin commenced the third educational experiment of which he was the subject, and was entered on the books of Christ's College in October 1827. So far as the direct results of the academic training thus received are concerned, the English University was not more successful than the Scottish. "During the three years which I spent at Cambridge my time was wasted, as far as the academical studies were concerned, as completely as at Edinburgh and as at school." (I, p. 46.) And yet, as before, there is ample evidence that this negative result cannot be put down to any native defect on the part of the scholar. Idle and dull young men, or even young men who being neither idle nor dull, are incapable of caring for anything but some hobby, do not devote themselves to the thorough study of Paley's 'Moral Philosophy,' and 'Evidences of Christianity'; nor are their reminiscences of this particular portion of their studies expressed in terms such as the following: "The logic of this book [the 'Evidences'] and, as I may add, of his 'Natural Theology' gave me as much delight as did Euclid." (I, p. 47.)

The collector's instinct, strong in Darwin from his childhood, as is usually the case in great naturalists, turned itself in the direction of Insects during his residence at Cambridge. In childhood, it had been

damped by the moral scruples of a sister, as to the propriety of catching and killing insects for the mere sake of possessing them, but now it broke out afresh, and Darwin became an enthusiastic beetle collector. Oddly enough he took no scientific interest in beetles, not even troubling himself to make out their names; his delight lay in the capture of a species which turned out to be rare or new, and still more in finding his name, as captor, recorded in print. Evidently, this beetle-hunting hobby had little to do with science, but was mainly a new phase of the old and undiminished love of sport. In the intervals of beetle-catching, when shooting and hunting were not to be had, riding across country answered the purpose. These tastes naturally threw the young undergraduate among a set of men who prefered hard riding to hard reading, and wasted the midnight oil upon other pursuits than that of academic distinction. A superficial observer might have had some grounds to fear that Dr. Darwin's wrathful prognosis might yet be verified. But if the eminently social tendencies of a vigorous and genial nature sought an outlet among a set of jovial sporting friends, there were other and no less strong proclivities which brought him into relation with associates of a very different stamp.

Though almost without ear and with a very defective memory for music, Darwin was so strongly and pleasurably affected by it that he became a member of a musical society; and an equal lack of natural capacity for drawing did not prevent him from studying good works of art with much care.

An acquaintance with even the rudiments of physical science was no part of the requirements for the ordinary Cambridge degree. But there were professors both of Geology and of Botany whose lectures were accessible to those who chose to attend them. The occupants of these chairs, in Darwin's time, were eminent men and also admirable lecturers in their widely different styles. The horror of geological lectures which Darwin had acquired at Edinburgh, unfortunately prevented him from going within reach of the fervid eloquence of Sedgwick; but he attended the botanical course, and though he paid no serious attention to the subject, he took great delight in the country excursions, which Henslow so well knew how to make both pleasant and instructive. The Botanical Professor was, in fact, a man of rare character and singularly extensive acquirements in all branches of natural history. It was his greatest pleasure to place his stores of knowledge at the disposal of the young men who gathered about him, and who found in him, not merely an encyclopedic teacher but a wise counseller, and, in case of worthiness, a warm friend. Darwin's acquaintance with him soon ripened into a friendship which was terminated only by Henslow's death in 1861, when *his quondam pupil* gave touching expression to his sense of what he

owed to one whom he calls (in one of his letters) his "dear old master in Natural History." (II, p. 217.) It was by Henslow's advice that Darwin was led to break the vow he had registered against making an acquaintance with geology; and it was through Henslow's good offices with Sedgwick that he obtained the opportunity of accompanying the Geological Professor on one of his excursions in Wales. He then received a certain amount of practical instruction in Geology, the value of which he subsequently warmly acknowledged. (I, p. 237.) In another direction, Henslow did him an immense, though not altogether intentional service, by recommending him to buy and study the recently published first volume of Lyell's 'Principles.' As an orthodox geologist of the then dominant catastrophic school, Henslow accompanied his recommendation with the admonition on no account to adopt Lyell's general views. But the warning fell on deaf ears, and it is hardly too much to say that Darwin's greatest work is the outcome of the unflinching application to Biology of the leading idea and the method applied in the 'Principles' to Geology.* Finally, it was through Henslow, and at his suggestion, that Darwin was offered the appointment to the "Beagle" as naturalist.

During the latter part of Darwin's residence at Cambridge the prospect of entering the Church, though the plan was never formally renounced, seems to have grown very shadowy. Humboldt's 'Personal Narrative,' and Herschel's 'Introduction to the Study of Natural Philosophy,' fell in his way and revealed to him his real vocation. The impression made by the former work was very strong. "My whole course of life," says Darwin in sending a message to Humboldt, "is due to having read and re-read, as a youth, his personal narrative." (I, p. 336.) The description of Teneriffe inspired Darwin with such a strong desire to visit the island, that he took some steps towards going there—inquiring about ships, and so on.

But, while this project was fermenting, Henslow, who had been asked to recommend a naturalist for Captain Fitzroy's projected expedition, at once thought of his pupil. In his letter of the 24th August, 1831, he says: "I have stated that I consider you to be the best qualified person I know of who is likely to undertake such a situation. I state this—not on the supposition of your being a *finished* naturalist, but as amply qualified for collecting, observing, and noting anything worthy to be noted in Natural History . . . . The voyage is to

* "After my return to England it appeared to me that by following the example of Lyell in Geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject [of the origin of species]." (I, p. 83.) See also the dedication of the second edition of the 'Journal of a Naturalist.'

t two years, and if you take plenty of books with you, anything ı please may be done." (I, p. 193.) The state of the case could not 'e been better put. Assuredly the young naturalist's theoretical l practical scientific training had gone no further than might ice for the outfit of an intelligent collector and notetaker. He s fully conscious of the fact, and his ambition hardly rose above hope that he should bring back materials for the scientific ons" at home of sufficient excellence to prevent them from ning and rending him. (I, p. 248.)

But a fourth educational experiment was to be tried. This time ture took him in hand herself and showed him the way by which, oorrow Henslow's prophetic phrase, "anything he pleased might done."

The conditions of life presented by a ship-of-war of only 242 tons then, would not, *primâ facie*, appear to be so favourable to intellec-l development as those offered by the cloistered retirement of rist's College. Darwin had not even a cabin to himself; while, in lition to the hindrances and interruptions incidental to sea-life, ich can be appreciated only by those who have had experience of m, sea-sickness came on whenever the little ship was "lively"; and, sidering the circumstances of the cruise, that must have been her mal state. Nevertheless, Darwin found on board the "Beagle" t which neither the pedagogues of Shrewsbury, nor the profes-iate of Edinburgh, nor the tutors of Cambridge had managed to e him. "I have always felt that I owe to the voyage the first real ining or education of my mind (I, p. 61);" and in a letter, written he was leaving England, he calls the voyage on which he was rting, with just insight, his "second life." (I, p. 214.) Happily Darwin's education, the school-time of the "Beagle" lasted five rs instead of two; and the countries which the ship visited were gularly well fitted to provide him with object-lessons on the nature things of the greatest value.

While at sea, he diligently collected, studied, and made copious es upon the surface Fauna. But with no previous training in section, hardly any power of drawing, and next to no knowledge comparative anatomy, his occupation with work of this kind—withstanding all his zeal and industry—resulted, for the most part, a vast accumulation of useless manuscript. Some acquaintance h the marine *Crustacea*, observations on *Planariæ* and on the quitous *Sagitta*, seem to have been the chief results of a great ount of labour in this direction.

It was otherwise with the terrestrial phenomena which came under voyager's notice: and Geology very soon took her revenge for the rn which the much-bored Edinburgh student had poured upon her. ee weeks after leaving England the ship touched land for the

first time at St. Jago, in the Cape de Verd Islands, and Darwin found his attention vividly engaged by the volcanic phenomena and the signs of upheaval which the island presented. His geological studies had already indicated the direction in which a great deal might be done, beyond collecting; and it was while sitting beneath a low lava cliff on the shore of this island, that a sense of his real capability first dawned upon Darwin, and prompted the ambition to write a book on the geology of the various countries visited. (I, p. 66.) Even at this early date, Darwin must have thought much on geological topics, for he was already convinced of the superiority of Lyell's views to those entertained by the catastrophists*; and his subsequent study of the tertiary deposits and of the terraced gravel beds of South America was eminently fitted to strengthen that conviction. The letters from South America contain little reference to any scientific topic except geology; and even the theory of the formation of coral reefs was prompted by the evidence of extensive and gradual changes of level afforded by the geology of South America; "No other work of mine," he says, "was begun in so deductive a spirit as this; for the whole theory was thought out on the West Coast of South America, before I had seen a true coral reef. I had, therefore, only to verify and extend my views by a careful examination of living reefs." (I, p. 70.) In 1835, when starting from Lima for the Galapagos, he recommends his friend, W. D. Fox, to take up geology:—"there is so much larger a field for thought than in the other branches of Natural History. I am become a zealous disciple of Mr. Lyell's views, as made known in his admirable book. Geologising in South America, I am tempted to carry parts to a greater extent even than he does. Geology is a capital science to begin with, as it requires nothing but a little reading, thinking, and hammering." (I, p. 263.) The truth of the last statement, when it was written, is a curious mark of the subsequent progress of geology. Even so late as 1836, Darwin speaks of being "much more inclined for geology than the other branches of Natural History." (I, p. 275.)

At the end of the letter to Mr. Fox, however, a little doubt is expressed whether zoological studies might not, after all, have been more profitable; and an interesting passage in the Autobiography enables us to understand the origin of this hesitation.

"During the voyage of the 'Beagle' I had been deeply impressed

* "I had brought with me the first volume of Lyell's 'Principles of Geology,' which I studied attentively; and the book was of the highest service to me in many ways. The very first place which I examined, namely, St. Jago in the Cape de Verd Islands, showed me clearly the wonderful superiority of Lyell's manner of treating Geology, compared with that of any other author whose works I had with me or ever afterwards read." (I, p. 62.)

r discovering in the Pampean formation great fossil animals covered ith armour like that on the existing armadillos; secondly, by the anner in which closely-allied animals replace one another in proeding southwards over the continent; and, thirdly, by the South merican character of most of the productions of the Galapagos rchipelago, and, more especially, by the manner in which they differ ightly on each island of the group: some of the islands appearing be very ancient in a geological sense.

"It was evident that such facts as these, as well as many others, ould only be explained on the supposition that species gradually ecome modified; and the subject haunted me. But it was equally rident that neither the action of the surrounding conditions, nor the ill of the organisms (especially in the case of plants) could account r the innumerable cases in which organisms of every kind are beaufully adapted to their habits of life; for instance, a woodpecker or tree-frog to climb trees, or a seed for dispersal by hooks or plumes. had always been much struck by such adaptations, and until these ould be explained it seemed to me almost useless to endeavour to rove by indirect evidence that species have been modified." (I, . 82.)

The facts to which reference is here made were, without doubt, minently fitted to attract the attention of a philosophical thinker; ut until the relations of the existing with the extinct species and of e species of the different geographical areas with one another were etermined with some exactness, they afforded but an unsafe foundaon for speculation. It was not possible that this determination hould have been effected before the return of the "Beagle" to ngland; and thus the date which Darwin (writing in 1837) assigns the dawn of the new light which was rising in his mind becomes itelligible.*

"In July opened first note-book on Transmutation of Species. Had een greatly struck from about the month of previous March on haracter of South American fossils and species on Galapagos rchipelago. These facts (especially latter) origin of all my views." [, p. 276.)

* I am indebted to Mr. F. Darwin for the knowledge of a letter addressed by his ther to Dr. Otto Zacharias in 1877, which contains the following paragraph, conrmatory of the view expressed above: "When I was on board the 'Beagle,' I elieved in the permanence of species, but, as far as I can remember, vague doubts ccasionally flitted across my mind. On my return home in the autumn of 1836 immediately began to prepare my journal for publication, and then saw how many cts indicated the common descent of species, so that in July, 1837, I opened a ote-book to record any facts which might bear on the question. But I did not ecome *convinced that species* were mutable until I think two or three years had *psed.*"

From March, 1837, then, Darwin, not without many misgivings and fluctuations of opinion, inclined towards transmutation as a provisional hypothesis. Three months afterwards he is hard at work collecting facts for the purpose of testing the hypothesis; and an almost apologetic passage in a letter to Lyell shows that, already, the attractions of biology are beginning to predominate over those of geology.

"I have lately been sadly tempted to be idle*—that is, as far as pure Geology is concerned—by the delightful number of new views which have been coming in thickly and steadily—on the classification and affinities and instincts of animals—bearing on the question of species. Note-book after note-book has been filled with facts which begin to group themselves *clearly* under sub-laws." (I, p. 298.)

The problem which was to be Darwin's chief subject of occupation for the rest of his life thus presented itself, at first, mainly under its distributional aspect. Why do species present certain relations in space and in time? Why are the animals and plants of the Galapagos Archipelago so like those of South America and yet different from them? Why are those of the several islets more or less different from one another? Why are the animals of the latest geological epoch in South America similar in *facies* to those which exist in the same region at the present day, and yet specifically or generically different?

The reply to these questions, which was almost universally received fifty years ago, was that animals and plants were created such as they are; and that their present distribution, at any rate so far as terrestrial organisms are concerned, has been effected by the migration of their ancestors from the region in which the ark stranded after the subsidence of the deluge. It is true that the geologists had drawn attention to a good many tolerably serious difficulties in the way of the diluvial part of this hypothesis, no less than to the supposition that the work of creation had occupied only a brief space of time. But even those, such as Lyell, who most strenuously argued in favour of the sufficiency of natural causes for the production of the phenomena of the inorganic world, held stoutly by the hypothesis of creation in the case of those of the world of life.

For persons who were unable to feel satisfied with the fashionable doctrine, there remained only two alternatives—the hypothesis of spontaneous generation, and that of descent with modification. The former was simply the creative hypothesis with the creator left out; the latter had already been propounded by De Maillet and Erasmus Darwin, among others, and, later, systematically expounded by

* Darwin generally uses the word "idle" in a peculiar sense. He means by it working hard at something he likes when he ought to be occupied with a less attractive subject. Though it sounds paradoxical, there is a good deal to be said in favour of this view of pleasant work.

Lamarck. But in the eyes of the naturalist of the "Beagle" (and, probably, in those of most sober thinkers), the advocates of transmutation had done the doctrine they expounded more harm than good.

Darwin's opinion of the scientific value of the 'Zoonomia' has already been mentioned. His verdict on Lamarck is given in the following passage of a letter to Lyell (March, 1863):—

"Lastly, you refer repeatedly to my view as a modification of Lamarck's doctrine of development and progression. If this is your deliberate opinion there is nothing to be said, but it does not seem so to me. Plato, Buffon, my grandfather, before Lamarck and others, propounded the *obvious* view that if species were not created separately they must have descended from other species, and I can see nothing else in common between the 'Origin' and Lamarck. I believe this way of putting the case is very injurious to its acceptance, as it implies necessary progression, and closely connects Wallace's and my views with what I consider, after two deliberate readings, as a wretched book, and one from which (I well remember to my surprise) I gained nothing."

"But," adds Darwin with a little touch of banter, "I know you rank it higher, which is curious, as it did not in the least shake your belief." (III, p. 14; see also p. 16, "to me it was an absolutely useless book.")

Unable to find any satisfactory theory of the process of descent with modification in the works of his predecessors, Darwin proceeded to lay the foundations of his own views independently; and he naturally turned, in the first place, to the only certainly known examples of descent with modification, namely, those which are presented by domestic animals and cultivated plants. He devoted himself to the study of these cases with a thoroughness to which none of his predecessors even remotely approximated; and he very soon had his reward in the discovery "that selection was the keystone of man's success in making useful races of animals and plants." (I, p. 83.)

This was the first step in Darwin's progress, though its immediate result was to bring him face to face with a great difficulty. "But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me." (I, p. 83.)

The key to this mystery was furnished by the accidental perusal of the famous essay of Malthus 'On Population' in the autumn of 1838. The necessary result of unrestricted multiplication is competition for the means of existence. The success of one competitor involves the failure of the rest, that is, their extinction; and this "selection" is dependent on the better adaptation of the successful competitor to the conditions of the competition. Variation occurs under natural, no *less than under artificial*, conditions. Unrestricted multiplication

implies the competition of varieties and the selection of those which are relatively best adapted to the conditions.

Neither Erasmus Darwin, nor Lamarck, had any inkling of the possibility of this process of "natural selection"; and though it had been foreshadowed by Wells in 1813, and more fully stated by Matthew in 1831, the speculations of the latter writer remained unknown to naturalists until after the publication of the 'Origin of Species.'

Darwin found in the doctrine of the selection of favourable variations by natural causes, which thus presented itself to his mind, not merely a probable theory of the origin of the diverse species of living forms, but that explanation of the phenomena of adaptation, which previous speculations had utterly failed to give. The process of natural selection is, in fact, dependent on adaptation—it is all one, whether one says that the competitor which survives is the "fittest" or the "best adapted." And it was a perfectly fair deduction that even the most complicated adaptations might result from the summation of a long series of simple favourable variations.

Darwin notes as a serious defect in the first sketch of his theory that he had omitted to consider one very important problem, the solution of which did not occur to him till some time afterwards. "This problem is the tendency in organic beings descended from the same stock to diverge in character as they become modified. . . . . The solution, as I believe, is that the modified offspring of all dominant and increasing forms tend to become adapted to many and highly diversified places in the economy of nature." (I, p. 84.)

It is curious that so much importance should be attached to this supplementary idea. It seems obvious that the theory of the origin of species by natural selection necessarily involves the divergence of the forms selected. An individual which varies, *ipso facto* diverges from the type of its species; and its progeny, in which the variation becomes intensified by selection, must diverge still more, not only from the parent stock, but from any other race of that stock starting from a variation of a different character. The selective process could not take place unless the selected variety was either better adapted to the conditions than the original stock, or adapted to other conditions than the original stock. In the first case, the original stock would be sooner or later extirpated; in the second, the type, as represented by the original stock and the variety, would occupy more diversified stations than it did before.

The theory, essentially such as it was published fourteen years later, was written out in 1844, and Darwin was so fully convinced of the importance of his work, as it then stood, that he made special arrangements for its publication in case of his death. But it is a singular example of reticent fortitude, that, although for the next

fourteen years the subject never left his mind, and during the latter half of that period he was constantly engaged in amassing facts bearing upon it from wide reading, a colossal correspondence, and a long series of experiments, only two or three friends were cognisant of his views. To the outside world he seemed to have his hands quite sufficiently full of other matters. In 1844, he published his observations on the volcanic islands visited during the voyage of the "Beagle." In 1845, a largely remodelled edition of his 'Journal' made its appearance, and immediately won, as it has ever since held, the favour of both the scientific and the unscientific public. In 1846, the 'Geological Observations in South America' came out, and this book was no sooner finished than Darwin set to work upon the Cirripedes. He was led to undertake this long and heavy task, partly by his desire to make out the relations of a very anomalous form which he had discovered on the coast of Chili; and, partly, by a sense of "presumption in accumulating facts and speculating on the subject of variation without, having worked out my due share of species." (II, p. 31.) The eight or nine years of labour, which resulted in a monograph of first-rate importance in systematic zoology (to say nothing of such novel points as the discovery of complemental males), left Darwin no room to reproach himself on this score, and few will share his "doubt whether the work was worth the consumption of so much time." (I, p. 82.)

In science no man can safely speculate about the nature and relation of things with which he is unacquainted at first hand, and the acquirement of an intimate and practical knowledge of the process of species-making and of all the uncertainties which underlie the boundaries between species and varieties, drawn by even the most careful and conscientious systematists* were of no less importance to the author of the 'Origin of Species' than was the bearing of the Cirripede work upon "the principles of a natural classification." (I, p. 81.) No one, as Darwin justly observes, has a "right to examine the question of species who has not minutely described many." (II, p. 39.)

In September, 1854, the Cirripede work was finished, "ten thousand barnacles" had been sent "out of the house, all over the world," and Darwin had the satisfaction of being free to turn again to his "old notes on species." In 1855, he began to breed pigeons, and to

* "After describing a set of forms as distinct species, tearing up my MS., and making them one species, tearing that up and making them separate, and then making them one again (which has happened to me), I have gnashed my teeth, cursed species, and asked what sin I had committed to be so punished." (II, p. 40.) Is there any naturalist provided with a logical sense and a large suite of specimens, who has not undergone pangs of the sort described in this vigorous paragraph, *which might, with advantage,* be printed on the title-page of every systematic *monograph as a warning to the uninitiated*?

make observations on the effects of use and disuse, experiments on seeds, and so on, while resuming his industrious collection of facts, with a view "to see how far they favour or are opposed to the notion that wild species are mutable or immutable. I mean with my utmost power to give all arguments and facts on both sides. I have a number of people helping me every way, and giving me most valuable assistance; but I often doubt whether the subject will not quite overpower me." (II, p. 49.)

Early in 1856, on Lyell's advice, Darwin began to write out his views on the origin of species on a scale three or four times as extensive as that of the work published in 1859. In July of the same year he gave a brief sketch of his theory in a letter to Asa Gray; and, in the year 1857, his letters to his correspondents show him to be busily engaged on what he calls his "big book." (II, pp. 85, 94.) In May, 1857, Darwin writes to Wallace: "I am now preparing my work [on the question how and in what way do species and varieties differ from each other] for publication, but I find the subject so very large, that, though I have written many chapters, I do not suppose I shall go to press for two years." (II, p. 95.) In December, 1857, he writes, in the course of a long letter to the same correspondent, "I am extremely glad to hear that you are attending to distribution in accordance with theoretical ideas. I am a firm believer that without speculation there is no good and original observation." (II, p. 108.)* In June, 1858, he received from Mr. Wallace, then in the Malay Archipelago, an 'Essay on the tendency of varieties to depart indefinitely from the original type,' of which Darwin says, "If Wallace had my MS. sketch written out in 1842 he could not have made a better short abstract! Even his terms stand now as heads of my chapters. Please return me the MS., which he does not say he wishes me to publish, but I shall, of course, at once write and offer to send it to any journal. So all my originality, whatever it may amount to, will be smashed, though my book, if ever it will have any value, will not be deteriorated; as all the labour consists in the application of the theory." (II, p. 116.)

Thus, Darwin's first impulse was to publish Wallace's essay without note or comment of his own. But, on consultation with Lyell and Hooker, the latter of whom had read the sketch of 1844, they suggested, as an undoubtedly more equitable course, that extracts from the MS. of 1844 and from the letter to Dr. Asa Gray should be communicated to the Linnean Society along with Wallace's essay. The joint communication was read on July 1, 1858, and published under the title 'On the Tendency of Species to form Varieties; and on the Perpetuation

* The last remark contains a pregnant truth, but it must be confessed it hardly squares with the declaration in the 'Autobiography' (I, p. 83) that he worked on *true Baconian* principles."

of Varieties and Species by Natural Means of Selection.' This was followed, on Darwin's part, by the composition of a summary account of the conclusions to which his twenty years' work on the species question had led him. It occupied him for thirteen months, and appeared in November, 1859, under the title 'On the Origin of Species* by means of Natural Selection or the Preservation of Favoured Races in the Struggle of Life.'

It is doubtful if any single book, except the 'Principia,' ever worked so great and so rapid a revolution in science, or made so deep an impression on the general mind. It aroused a tempest of opposition and met with equally vehement support, and it must be added that no book has been more widely and persistently misunderstood by both friends and foes. In 1861, Darwin remarks to a correspondent, "you understand my book perfectly, and that I find a very rare event with my critics." (I, p. 313.) The immense popularity which the 'Origin' at once acquired was no doubt largely due to its many points of contact with philosophical and theological questions in which every intelligent man feels a profound interest; but a good deal must be assigned to a somewhat delusive simplicity of style, which tends to disguise the complexity and difficulty of the subject, and much to the wealth of information on all sorts of curious problems of natural history, which is made accessible to the most unlearned reader. But long occupation with the work has led the present writer to believe that the 'Origin of Species' is one of the hardest of books to master;* and he is justified in this conviction by observing that although the 'Origin' has been close on thirty years before the world, the strangest misconceptions of the essential nature of the theory therein advocated are still put forth by serious writers.

Although, then, the present occasion is not suitable for any detailed criticism of the theory, or of the objections which have been brought against it, it may not be out of place to endeavour to separate the substance of the theory from its accidents; and to show that a variety not only of hostile comments, but of friendly would-be improvements lose their *raison d'être* to the careful student. Observation proves the existence among all living beings of phenomena of three kinds, denoted by the terms heredity, variation, and multiplication. Progeny tend to resemble their parents; nevertheless all their organs and functions are susceptible of departing more or less from the average parental character; and their number is in excess of that of their parents. Severe competition for the means of living, or the struggle for existence, is a necessary consequence of unlimited multiplication; while selection, or the preservation of favourable

* He is comforted to find that probably the best qualified judge among all the *readers of the 'Origin' in 1859* was of the same opinion. Sir J. Hooker writes "*it is the very hardest book to read*, to full profit, that I ever tried." (II, p. 242.)

variations and the extinction of others, is a necessary consequence of severe competition. "Favourable variations" are those which are better adapted to surrounding conditions. It follows, therefore, that every variety which is selected into a species is so favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals. In other words, every species which exists, exists in virtue of adaptation, and whatever accounts for that adaptation accounts for the existence of the species.

To say that Darwin has put forward a theory of the adaptation of species, but not of their origin, is therefore to misunderstand the first principles of the theory. For, as has been pointed out, it is a necessary consequence of the theory of selection that every species must have some one or more structural or functional peculiarities, in virtue of the advantage conferred by which, it has fought through the crowd of its competitors and achieved a certain duration. In this sense, it is true that every species has been "originated" by selection.

There is another sense, however, in which it is equally true that selection originates nothing. "Unless profitable variations . . . . . occur natural selection can do nothing" ('Origin,' Ed. I, p. 82). "Nothing can be effected unless favourable variations occur" (*ibid.*, p. 108). "What applies to one animal will apply throughout time to all animals—that is, if they vary—for otherwise natural selection can do nothing. So it will be with plants" (*ibid.*, p. 113). Strictly speaking, therefore, the origin of species in general lies in variation; while the origin of any particular species lies, firstly, in the occurrence, and secondly, in the selection and preservation of a particular variation. Clearness on this head will relieve one from the necessity of attending to the fallacious assertion that natural selection is a *deus ex machinâ*, or occult agency.

Those, again, who confuse the operation of the natural causes which bring about variation and selection with what they are pleased to call "chance" can hardly have read the opening paragraph of the fifth chapter of the 'Origin' (Ed. I, p. 131): "I have sometimes spoken as if the variations . . . . had been due to chance. This is of course a wholly incorrect expression, but it seems to acknowledge plainly our ignorance of the cause of each particular variation."

Another point of great importance to the right comprehension of the theory, is, that while every species must needs have some adaptive advantageous characters to which it owes its preservation by selection, it may possess any number of others which are neither advantageous nor disadvantageous, but indifferent, or even slightly disadvantageous. (*Ibid.*, p. 81.) For variations take place, not merely in one organ or function at a time, but in many; and thus

[redacted]



dvantageous variation, which gives rise to the selection of a new or species, may be accompanied by others which are indifferent, which are just as strongly hereditary as the advantageous varia- . The advantageous structure is but one product of a modified ral constitution which may manifest itself by several other acts; and the selective process carries the general constitution g with the advantageous special peculiarity. A given species of may owe its existence to the selective adaptation of its flowers sect fertilisers; but the character of its leaves may be the result riations of an indifferent character. It is the origin of varia- of this kind to which Darwin refers in his frequent reference to he calls "laws of correlation of growth" or "correlated tion."

iese considerations lead us further to see the inappropriateness of bjections raised to Darwin's theory on the ground that natural selec- does not account for the first commencements of useful organs. it does not pretend to do so. The source of such commencements cessarily to be sought in indifferent variations, which remain fected by selection until they have taken such a form as to me utilisable in the struggle for existence.

is not essential to Darwin's theory that anything more should be ned than the facts of heredity, variation, and unlimited multipli- n; and the validity of the deductive reasoning as to the effect of last (that is, of the struggle for existence which it involves) the varieties resulting from the operation of the former. Nor essential that one should take up any particular position in rd to the mode of variation, whether, for example, it takes *per saltum* or gradually; whether it is definite in character ndefinite. Still less are those who accept the theory bound y particular views as to the causes of heredity or of variation.

iat Darwin held strong opinions on some or all of these points be quite true; but, so far as the theory is concerned, they must be rded as *obiter dicta.* With respect to the causes of variation, vin's opinions are, from first to last, put forward altogether tenta- y. In the first edition of the 'Origin,' he attributes the igest influence to changes in the conditions of life of parental nisms, which he appears to think act on the germ through intermediation of the sexual organs. He points out, over and again, that habit, use, disuse, and the direct influence of con- ns have some effect, but he does not think it great, and he draws ition to the difficulty of distinguishing between effects of these cies and those of selection. There is, however, one class of itions which he withdraws from the direct influence of selection, ely, *the variations in the fertility of the sexual union of more or losely allied forms.* He regards less fertility, or more or less

complete sterility, as "incidental to other acquired differences." (*Ibid.*, p. 245.)

Considering the difficulties which surround the question of the causes of variation, it is not to be wondered at, that Darwin should have inclined, sometimes, rather more to one and, sometimes, rather more to another of the possible alternatives. There is little difference between the last edition of the 'Origin' (1872) and the first on this head. In 1876, however, he writes to Moritz Wagner, "In my opinion, the greatest error which I have committed has been not allowing sufficient weight to the direct action of the environment, *i.e.*, food, climate, &c., independently of natural selection . . . . When I wrote the 'Origin,' and for some years afterwards, I could find little good evidence of the direct action of the environment; now there is a large body of evidence, and your case of the Saturnia is one of the most remarkable of which I have heard." (III, p. 159.) But there is really nothing to prevent the most tenacious adherent to the theory of natural selection from taking any view he pleases as to the importance of the direct influence of conditions and the hereditary transmissibility of the modifications which they produce. In fact, there is a good deal to be said for the view that the so-called direct influence of conditions is itself a case of selection. Whether the hypothesis of Pangenesis be accepted or rejected, it can hardly be doubted that the struggle for existence goes on not merely between distinct organisms, but between the physiological units of which each organism is composed, and that changes in external conditions favour some and hinder others.

After a short stay in Cambridge, Darwin resided in London for the first five years which followed his return to England; and for three years, he held the post of Secretary to the Geological Society, though he shared to the full his friend Lyell's objection to entanglement in such engagements. In fact, he used to say in later life, more than half in earnest, that he gave up hoping for work from men who accepted official duties and, especially, Government appointments. Happily for him he was exempted from the necessity of making any sacrifice of this kind, but an even heavier burden was laid upon him. During the earlier half of his voyage Darwin retained the vigorous health of his boyhood, and indeed proved himself to be exceptionally capable of enduring fatigue and privation. An anomalous but severe disorder, which laid him up for several weeks at Valparaiso in 1834, however, seems to have left its mark on his constitution; and, in the later years of his London life, attacks of illness, usually accompanied by severe vomiting and great prostration of strength, became frequent. As he grew older, a considerable part of every day, even at his best times, was spent in misery; while, not

unfrequently, months of suffering rendered work of any kind impossible. Even Darwin's remarkable tenacity of purpose and methodical utilisation of every particle of available energy could not have enabled him to achieve a fraction of the vast amount of labour he got through, in the course of the following forty years, had not the wisest and the most loving care unceasingly surrounded him from the time of his marriage in 1839. As early as 1842, the failure of health was so marked that removal from London became imperatively necessary; and Darwin purchased a house and grounds at Down, a solitary hamlet in Kent, which was his home for the rest of his life. Under the strictly regulated conditions of a valetudinarian existence, the intellectual activity of the invalid might have put to shame most healthy men; and, so long as he could hold his head up, there was no limit to the genial kindness of thought and action for all about him. Those friends who were privileged to share the intimate life of the household at Down have an abiding memory of the cheerful restfulness which pervaded and characterised it.

After mentioning his settlement at Down, Darwin writes in his Autobiography:—

"My chief enjoyment and sole employment throughout life has been scientific work; and the excitement from such work makes me, for the time, forget, or drives quite away, my daily discomfort. I have, therefore, nothing to record during the last of my life except the publication of my several books." (I, p. 79.)

Of such works published subsequently to 1859, several are monographic discussions of topics briefly dealt with in the 'Origin,' which, it must always be recollected, was considered by the author to be merely an abstract of an *opus majus*.

The earliest of the books which may be placed in this category, 'On the Various Contrivances by which Orchids are Fertilised by Insects,' was published in 1862, and whether we regard its theoretical significance, the excellence of the observations and the ingenuity of the reasonings which it records, or the prodigious mass of subsequent investigation of which it has been the parent, it has no superior in point of importance. The conviction that no theory of the origin of species could be satisfactory which failed to offer an explanation of the way in which mechanisms involving adaptations of structure and function to the performance of certain operations are brought about, was, from the first, dominant in Darwin's mind. As has been seen, he rejected Lamarck's views because of their obvious incapacity to furnish such an explanation in the case of the great majority of animal mechanisms, and in that of all those presented by the vegetable world.

So far back as 1793, the wonderful work of Sprengel had established, beyond any reasonable doubt, the fact that, in a large nur

of cases, a flower is a piece of mechanism the object of which is to convert insect visitors into agents of fertilisation. Sprengel's observations had been most undeservedly neglected and well-nigh forgotten; but Robert Brown having directed Darwin's attention to them in 1841, he was attracted towards the subject, and verified many of Sprengel's statements. (III, p. 258.) It may be doubted whether there was a living botanical specialist, except perhaps Brown, who had done as much. If, however, adaptations of this kind were to be explained by natural selection, it was necessary to show that the plants which were provided with mechanisms for ensuring the aid of insects as fertilisers, were by so much the better fitted to compete with their rivals. This Sprengel had not done. Darwin had been attending to cross fertilisation in plants, so far back as 1839, from having arrived in the course of his speculations on the origin of species "that crossing played an important part in keeping specific forms constant" (I, p. 90). The further development of his views on the importance of cross fertilisation appears to have taken place between this time and 1857, when he published his first papers on the fertilisation of flowers in the 'Gardener's Chronicle.' If the conclusion at which he ultimately arrived, that cross fertilisation is favourable to the fertility of the parent and to the vigour of the offspring, is correct, then it follows that all those mechanisms which hinder self-fertilisation and favour crossing must be advantageous in the struggle for existence; and, the more perfect the action of the mechanism, the greater the advantage. Thus the way lay open for the operation of natural selection in gradually perfecting the flower as a fertilisation-trap. Analogous reasoning applies to the fertilising insect. The better its structure is adapted to that of the trap, the more will it be able to profit by the bait, whether of honey or of pollen, to the exclusion of its competitors. Thus, by a sort of action and reaction, a two-fold series of adaptive modifications will be brought about.

In 1865, the important bearing of this subject on his theory led Darwin to commence a great series of laborious and difficult experiments on the fertilisation of plants, which occupied him for eleven years, and furnished him with the unexpectedly strong evidence in favour of the influence of crossing which he published in 1876, under the title of 'The Effects of Cross and Self Fertilisation in the Vegetable Kingdom.' Incidentally, as it were, to this heavy piece of work, he made the remarkable series of observations on the different arrangements by which crossing is favoured and, in many cases, necessitated, which appeared in the work on 'The Different Forms of Flowers in Plants of the same Species' in 1877.

In the course of the twenty years during which Darwin was thus occupied in opening up new regions of investigation to the botanist nd showing the profound physiological significance of the apparently

meaningless diversities of floral structure, his attention was keenly alive to any other interesting phenomena of plant life which came in his way. In his correspondence, he not unfrequently laughs at himself for his ignorance of systematic botany; and his acquaintance with vegetable anatomy and physiology was of the slenderest. Nevertheless, if any of the less common features of plant life came under his notice, that imperious necessity of seeking for causes which nature had laid upon him, impelled, and indeed compelled, him to inquire the how and the why of the fact, and its bearing on his general views. And as, happily, the atavic tendency to frame hypothèses was accompanied by an equally strong need to test them by well-devised experiments, and to acquire all possible information before publishing his results, the effect was that he touched no topic without elucidating it.

Thus the investigation of the operations of insectivorous plants, embodied in the work on that topic published in 1875, was started fifteen years before, by a passing observation made during one of Darwin's rare holidays.

"In the summer of 1860, I was idling and resting near Hartfield, where two species of Drosera abound; and I noticed that numerous insects had been entrapped by the leaves. I carried home some plants, and on giving them some insects saw the movements of the tentacles, and this made me think it possible that the insects were caught for some special purpose. Fortunately, a crucial test occurred to me, that of placing a large number of leaves in various nitrogenous and non-nitrogenous fluids of equal density; and as soon as I found that the former alone excited energetic movements, it was obvious that here was a fine new field for investigation." (I, p. 95.)

The researches thus initiated led to the proof that plants are capable of secreting a digestive fluid like that of animals, and of profiting by the result of digestion; whereby the peculiar apparatuses of the insectivorous plants were brought within the scope of natural selection. Moreover, these inquiries widely enlarged our knowledge of the manner in which stimuli are transmitted in plants, and opened up a prospect of drawing closer the analogies between the motor process of plants and those of animals.

So with respect to the books on 'Climbing Plants' (1875), and on the 'Power of Movement in Plants' (1880), Darwin says;—

"I was led to take up this subject by reading a short paper by Asa Gray, published in 1858. He sent me some seeds, and on raising some plants I was so much fascinated and perplexed by the revolving movements of the tendrils and stems, which movements are really very simple, though appearing at first sight very complex, that I procured various other kinds of climbing plants and studied the whole *subject . . . . Some of the adaptations displayed by climbing plants*

are as beautiful as those of orchids for ensuring cross-fertilisation." (I, p. 93.)

In the midst of all this amount of work, remarkable alike for its variety and its importance, among plants, the animal kingdom was by no means neglected. A large moiety of 'The Variation of Animals and Plants under Domestication' (1868), which contains the *pièces justificatives* of the first chapter of the 'Origin,' is devotod to domestic animals, and the hypothesis of 'pangenesis' propounded in the second volume applies to the whole living world. In the 'Origin' Darwin throws out some suggestions as to the causes of variation, but he takes heredity, as it is manifested by individual organisms, for granted, as an ultimate fact; pangenesis is an attempt to account for the phenomena of heredity in the organism, on the assumption that the physiological units of which the organism is composed give off gemmules, which, in virtue of heredity, tend to reproduce the unit from which they are derived.

That Darwin had the application of his theory to the origin of the human species clearly in his mind in 1859, is obvious from a passage in the first edition of 'The Origin of Species.' (Ed. 1, p. 488.) "In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by graduation. Light will be thrown on the origin of man and his history." It is one of the curiosities of scientific literature, that, in the face of this plain declaration, its author should have been charged with concealing his opinions on the subject of the origin of man. But he reserved the full statement of his views until 1871, when the 'Descent of Man' was published. The 'Expression of the Emotions' (originally intended to form only a chapter in the 'Descent of Man') grew into a separate volume, which appeared in 1872. Although always taking a keen interest in geology, Darwin naturally found no time disposable for geological work, even had his health permitted it, after he became seriously engaged with the great problem of species. But the last of his labours is, in some sense, a return to his earliest, inasmuch as it is an expansion of a short paper read before the Geological Society more than forty years before, and, as he says, "revived old geological thoughts" (I, p. 98). In fact, 'The Formation of Vegetable Mould through the Action of Worms,' affords as striking an example of the great results produced by the long continued operation of small causes as even the author of the 'Principles of Geology' could have desired.

In the early months of 1882 Darwin's health underwent a change for the worse; attacks of giddiness and fainting supervened, and on the 19th of April he died. On the 24th, his remains were interred in Westminster Abbey, in accordance with the general feeling that such

a man as he should not go to the grave without some public recognition of the greatness of his work.

Mr. Darwin became a Fellow of the Royal Society in 1839; one of the Royal Medals was awarded to him in 1853, and he received the Copley Medal in 1864. The 'Life and Letters,' edited with admirable skill and judgment by Mr. Francis Darwin, gives a full and singularly vivid presentment of his father's personal character, of his mode of work, and of the events of his life. In the present brief obituary notice, the writer has attempted nothing more than to select and put together those facts which enable us to trace the intellectual evolution of one of the greatest of the many great men of science whose names adorn the long roll of the Fellows of the Royal Society.

T. H. H.

Mr. Thomas Blizard Curling, F.R.C.S., F.R.S., died at Cannes, on the 4th of March, 1888, in the 78th year of age, of a severe attack of pneumonia or congestion of the lungs, caused by chill.

This distinguished surgeon was born in 1811, and resided in London during the greater part of his professional life, which was one of continued scientific and public utility. The value of his contributions to surgery and pathology, his great eminence as a surgeon and clinical teacher, and his upright, just, and honourable character, not only placed him in the foremost rank of his profession, but secured for him the affection and esteem of the numerous friends who deplore his loss.

Mr. Curling had retired from the active duties of his profession as Senior Surgeon of the London Hospital in 1869, but continued to practise until within the last ten years, which were spent in well-earned rest at Brighton, varied by occasional visits to the Riviera, where, as has been stated, his career was brought to a sudden close by a severe pulmonary attack. He obtained professional distinction early in life; at the age of twenty-two he was appointed Assistant-Surgeon of the London Hospital; in this he appears to have been partly aided by the influence of his uncle, Sir W. Blizard, who thus happily had the means of placing the opportunity of advancement, which was so readily seized and so fully utilised, within the grasp of the young surgeon whose brilliant subsequent career proved how justly its early promise had been estimated by those who appointed him to so important a post.

Mr. Curling's career as a hospital surgeon and teacher of surgery was one of continued progress and success. A recent notice of him says:—"Perhaps nowhere was his character more apparent than while *ward-visiting at the* London Hospital. His methodical and punctu

habits passed into a proverb with the students, for he usually entered the gates as his visiting hour struck. A strict disciplinarian, he was a terror to the slovenly dresser, but an object of respect and admiration to the zealous. Freely blaming, if blame were due, he never withheld praise when such was deserved. Exact and punctilious in detail himself, he evinced his strong sense of duty to the patient by examining into the smallest minutiæ of dressing and note-taking. Possessing a sound and well-balanced judgment, backed by great clinical experience, he did not permit theories to be based on insufficient bases. His practice and his teaching were not at variance; both were sound, upright, and just."

Mr. Curling was appointed Lecturer on Surgery in 1846, and became Full Surgeon of the London Hospital in 1849, from which office he retired in 1869, retaining that of Consulting Surgeon till his death. He was appointed Examiner in Surgery to the London University in 1859, Member of the Council of the Royal College of Surgeons in 1864, Examiner in 1871, and filled the high office of President in 1873. He had been elected a Fellow of the Royal Society as early as 1850.

His large and varied experience is stamped on his written works. His earliest investigations were on tetanus, which were rewarded by the Jacksonian prize in 1834. This sound work was followed by many communications of interest and importance to the Royal Medico-Chirurgical and Pathological Societies, comprising amongst them that upon Duodenal Ulceration as a consequence of burns. Towards 1855 the subject of his articles tended rather to the illustration of diseases of the testes and rectum, and his wide experience in these sections of surgery was of much benefit to those who suffered from these affections. His works on Diseases of the Testis and on Diseases of the Rectum, each of which reached a fourth edition, are standard authorities on the subjects of which they treat.

He was a most courteous, amiable man; undemonstrative in manner, but sincere and true in his friendships and feelings. His character has justly been described as "one of singular honesty and straightforwardness; he had a kind heart, and secured and kept the deep respect of all who knew him."

Mr. Curling had two sons, both of whom, as well as their mother, predeceased him.

J. F.

By the death of PHILIP HENRY GOSSE the Society has lost not only a many-sided and experienced naturalist, but one who did more than almost any of his scientific contemporaries to popularise the study of natural objects.

Mr. Gosse was born at Worcester in 1810—his father, a miniature painter of some note in his day. He was educated, in part at least, at the Blandford Grammar School, and at seventeen was sent out to Newfoundland as a clerk in a business house. After eight years of commercial life he settled in Canada as a farmer, but the venture did not prove successful, and he returned to England. In 1838 he went south through the United States, and was engaged as a schoolmaster in Alabama; subsequently he resided for some time in Jamaica as a professional naturalist, and then, having definitely adopted natural history and literature as a profession, he returned to settle in England. The roving life of his earlier years afforded wide opportunities for natural history pursuits, and his early works show evidence at least of acute powers of observation. 'The Canadian Naturalist' (1840) and 'The Birds of Jamaica' (1851) were perhaps his most important contributions during this period, but he had published also a number of zoological manuals and other books of more popular character.

From this time, however, Mr. Gosse devoted himself more particularly to the British marine fauna and flora. He was an assiduous collector and, simultaneously perhaps with the late Mr. Warrington, devised the marine aquarium, as a means of observing the habits and economy of marine shallow-water organisms. The idea was taken up by the Zoological Society, who, in 1853, constructed tanks on a considerable scale in their gardens in Regent's Park. 'A Naturalist's Rambles on the Devonshire Coast,' a little handbook to 'The Aquarium' (1853–4), and other works of similar bearing published about the same time, attracted much attention and, as a practical result, aquaria became common, and the collection of objects for them a popular sea-side amusement. Of greater importance from a scientific point of view was his 'Manual of Marine Zoology' (1855–6); two small volumes, copiously illustrated with outline drawings—a work extremely useful in its day.

Mr. Gosse's subsequent contributions to scientific literature were less frequent but of more original character. His name will probably be best remembered as the author of the 'Actinologia Britannica,' a history of the sea-anemones and corals of the British Islands, which still, after the lapse of nearly thirty years, maintains its authoritative position. Of later times his attention was more particularly directed to the Rotifera, and the results of his observations up to 1886 were embodied in an *important* monograph of the group, published con-*jointly with Dr. C. T.* Hudson. The Society's 'Catalogue of

Scientific Papers' contains a list of nearly sixty memoirs from his pen between 1843 and 1867, and this number would require considerable addition to include those of recent years. But the bulk of his literary labour was expended on works of more popular nature. These were very numerous, and embraced a great variety of subjects; the style was generally very happily chosen, and they were marked by the same accuracy as his more strictly scientific writings. Much of the interest and value of Mr. Gosse's contributions to science is due to their admirable illustration, the author's facility and precision with pencil and brush, which lasted late into old age, being no doubt in part an inherited gift.

Mr. Gosse was elected a Fellow of the Society in 1856. His decease took place at Marychurch on the 23rd of April, 1888, in his 79th year. For many years he had led a secluded life, of which his friends were kept aware by his occasional contributions to the scientific journals.

H. B. B.

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